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ART. I.—*Photometric Experiments.* PART SECOND.—*On the amount of light transmitted by plates of polished crown glass at a perpendicular incidence;* by OGDEN N. ROOD, Professor of Physics in Columbia College.

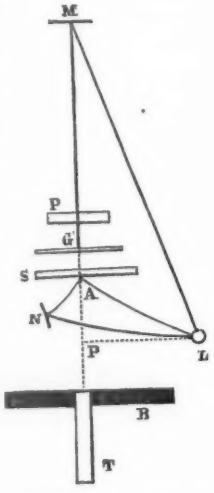
IT would seem that in the direct photometric experiments on this matter, the instrumental means employed have been more or less defective, in consequence of which the results gained, although having a certain amount of general correctness, still leave much to be desired. The main sources of error have been, first, the use of a defective mode of making the compensations, not allowing the experimenter to take full advantage of the sensitiveness of the eye, and secondly the employment of *two sources* of light, which always brings with it another set of unavoidable errors. Apart from this, in cases when the amount of reflected or transmitted light has been thus approximately obtained, the indices of refraction of the substances used do not seem to have been determined, so that for *perpendicular incidence*, slender data exist for comparing the results of theory and experiment.

It will be seen in what follows, that, to avoid the variability caused by two sources of light, I have devised a method in which only a single gas-flame is employed, the light being divided in such a way that a certain portion of it always illuminates the posterior side of the screen, while the other portion is reflected from a movable mirror, and pursues its way unobstructed to the screen, or is transmitted to it by the plate under experiment. The compensation is made by moving the mirror,

and when once effected of course remains undisturbed by fluctuations of the flame. In addition to this it will also be observed that a more delicate screen, constructed, however, on the general principles indicated in the first part of this paper, has been employed, and these modifications, taken in connection with certain precautions described below, have given the results an accuracy superior to that attained in the experiments on silver mirrors.

Arrangement of the photometer with a single gas flame for experiments on the amount of light transmitted by plates of various substances.

The source of light is a small gas flame at L, about an inch in height, the gas flowing as before from a plain circular opening. A portion of its light, L A, directly illuminates the screen S, while another portion N A is reflected on the screen by a Liebig's mirror, one protected by a coating of copper being selected: this equalizes the light on both sides of the bare "spot" in S. A third portion of the light from L falls on M, a mirror like that just mentioned, and there is reflected perpendicularly through P, the plate under examination, on G the ground glass. It will be observed that the path pursued by the light in this latter case is the distance L M+M G: by the graduation on the instrument M G is actually measured, G P is known; also P L; hence we have the means of calculating the distance M L, which is to be added to M G, giving the distance required. As these calculations involve some labor, I constructed for my instrument a table by which the *measured* distance G M, is readily converted into the *total* distance.



The photometer was the same as that described in the first part of this paper, the following changes having been introduced: the "spot" on the screen was made much smaller, being only about $\frac{1}{30}$ of an inch in horizontal diameter, with a length about three times as great; it was observed by a small telescope T, magnifying six diameters. This reduction in the size of the "spot" rendered it possible to illuminate the ground surrounding it in a manner beyond reproach.

Mode of adjusting the apparatus for experiment.

1st. The center of the flame at L, and the center of the mirror M, must be at the same height above the common base.

2nd. The screen and plates to be examined are to be made perpendicular to the axis of the instrument, by the aid of the small gas flame mentioned in the first part of this paper: the telescope T is also most readily collimated by the use of the same flame, the screen, &c., having been temporarily removed.

3rd. The mirror M evidently must be brought into such a position that it shall send the ray MP perpendicularly to P, or along the axis of the instrument when its distance is such as to effect compensation; for if the reflected ray were sent obliquely through the ground glass plate G, noticeable errors would be introduced. This adjustment, which is important if good results are expected, is best effected by making one or two compensations so as to determine approximately the correct distance of the mirror, which is then rotated on its vertical axis so that the reflected image of the flame is made to fall in the center of the field of the telescope, the screen having been previously removed. By a repetition of this operation finally the mirror is brought into its proper position, when a *single* series of measurements can be made, the minute differences in the individual compensations introducing no appreciable errors. When, however, the compensation point itself has been shifted by the introduction or removal of a plate of glass, the mirror will correspondingly be moved away or toward S, and of course the ray MP will be sent a little to the right or left, and it becomes necessary to devise some simple way in which this difficulty can be obviated, without in each instance removing the screen and altering the focus of the telescope.

The mirror then being actually in adjustment, this, when lost, can be recovered as follows: in the screen B about four inches above the telescope is a circular aperture $\frac{1}{10}$ of an inch in width; the reflection of the flame in the mirror is observed through it with the naked eye, and the image of the flame is seen higher up on the mirror, and is made to coincide with a short black line drawn there previously. The ray MP can always thus be made to coincide with the axis of the instrument, it not even being necessary for the observer to leave his seat or diminish the sensitiveness of the eye by exposure. I am particular in describing this precaution, having in my own case rejected the results of 1300 compensations which were made with a comparative lack of attention to this single point.

Mode of measuring the amount of light transmitted by a plate of glass, &c.

1st. The plate is placed at P and collimated by the use of the small gas flame.

2nd. A compensation is effected by moving the mirror.

3rd. The mirror is adjusted by the use of the aperture in B, and another compensation made, followed by a second adjustment of the mirror, if necessary.

4th. A series of careful compensations are now made, *alternately by the approach and recession of the mirror;* these are registered on the fillet of paper.

5th. The plate is removed without the experimenter changing position or exposing the eye to bright light, and a compensation is made which necessitates of course a new adjustment of the mirror by the aid of the aperture in the screen B. Finally, a series of compensations are effected with the free flame and registered.

At this point it may be well to notice an objection which might be urged to this mode of experimenting, viz: it is evident that the angles at which the light is reflected from the mirror will not be identical in the presence and absence of the plate of glass, and as the amount of light reflected by glass and silver varies with the angle of reflection, this might become a source of error and necessitate a correction. With the plates of colorless glass employed by me, however, the angle in one case was 5° 30′ and in the other 5° 18′, the difference of 12′ being much too small to produce an appreciable effect, as we know from the results of older experiments.

For the purpose of showing what can be effected by this method, I can perhaps do no better than give the results of five sets of experiments; each of them consisted of four sets of eight double, or sixteen single compensations, and during the progress of each trial the conditions remained the same. The experiments were made as follows: eight double compensations were effected with the photometer and registered; then everything being arranged as before, a new set of compensations were made, &c., all of which was repeated four times. The arithmetical mean of the distances of the source of light from the screen was then obtained separately for each of the four experiments, and from these four mean values of the distance, a final mean value was deduced. This last quantity was then squared and compared separately with the squares of each of the first four mean distances, which proceeding tells at once the difference of the four results from the final mean result, in per cents or fractions of a per cent, and is adapted to give an idea of the degree of accuracy to be expected in this kind of observation.

Differences from mean in per cents:

No. 1.	No. 2.	No. 3.	No. 4.	No. 5.
+·012	−·275	−·252	+·573	−·102
+·347	+·126	−·137	−·208	+·057
+·158	−·124	+·385	−·060	+·041
−·512	+·275	+·007	−·308	+·005

It will be observed that in the twenty experiments, only two cases occur in which the difference rises slightly above one half per cent, or above $\frac{1}{200}$ of the whole quantity of light, while in nearly all the other cases the difference falls considerably below this quantity, and the average of their differences is less than 0·2 per cent, or less than $\frac{1}{500}$ of the whole amount.

As the reliability of measurements with a photometer is evidently connected with the power of the eye in distinguishing different degrees of brightness in adjacent surfaces, it is well to review here briefly the results that have been obtained by different observers, as to the sensitiveness of the eye under favorable conditions.

The least difference which Boguer with his method, (two shadows,) was able to distinguish was $\frac{1}{64}$ of the whole; Fechner's friends using the same method were able to reduce this quantity to $\frac{1}{160}$. Arago remarked that where one of the illuminated surfaces was in motion smaller differences could be perceived, and was able under favorable circumstances to distinguish $\frac{1}{131}$, while Masson, who used revolving discs, along with his friends, was able to distinguish between $\frac{1}{50}$ to $\frac{1}{120}$ of the whole quantity of light. Near a window Helmholtz was able to distinguish with certainty a difference of $\frac{1}{133}$, and occasionally as small a quantity as $\frac{1}{167}$, while in the middle of a room he was able to appreciate certainly only $\frac{1}{117}$, seldom and uncertainly $\frac{1}{133}$.*

In all the better of the above mentioned experiments, photometers in any proper sense of the term were not employed, the best of them being made by the use of revolving discs of white paper furnished with narrow black sectors, which are calculated to discover the absolute sensitiveness of the eye under the most favorable conditions. It must not however be imagined that because a good eye can distinguish a difference of $\frac{1}{120}$ on a revolving disc, that the same eye would be able to obtain a similar result with ordinary forms of the photometer, where the conditions are far less favorable, and accordingly it seemed of interest to me to determine by a series of experiments the average sensitiveness of the eye while using the photometric method described in this paper, particularly as the plan of using

* Physiologische Optik, von H. Helmholtz, page 311.

a single flame and mirror offered great facilities for making these observations in a reliable manner. The following mode was employed: the light from the mirror was allowed to fall on the unobstructed screen, and the mirror was *drawn up* till the spot had disappeared, when the observation was registered; the next compensation was made by *pushing* the mirror *away*, and so on alternately, *the compensations by pull and push being registered on separate fillets of paper by two pens.* In the first experiment fourteen compensations were made by the advancing, and an equal number by the retreating mirror, and the mean of these twenty eight quantities was taken as the true distance of the mirror from the screen; then on comparing the mean of either set of compensations with this true distance, it was easy to ascertain how great a difference from the truth had been tolerated by the eye under the given circumstances. In the first experiment this quantity was found to be $\frac{1}{114}$, in a second similar trial $\frac{1}{125}$ of the total amount of light. The "screen" in these experiments was quite *new*, and an inferior result was obtained by using a "screen," which had for six weeks been exposed quite unprotected to the action of the air; here the usual darkening of the edges had begun and progressed so far as to be faintly visible during compensation.

In the experiments just detailed the highest *average* sensitiveness of the eye is only $\frac{1}{125}$, while as before shown in practical use the average difference from the mean is less than $\frac{1}{5}\frac{1}{0}\frac{1}{0}$ of the whole. This higher degree of accuracy results naturally from the mode in which the compensations are made, (alternately by approach and recession,) and the fact that the compensations are thus interwoven, in such a manner as to eliminate errors introduced by the slightly varying sensibility of the eye.

Experiments on the amount of light transmitted by plates of glass at a perpendicular incidence.

A knowledge of the amount of light transmitted and reflected by colorless transparent substances has a certain degree of interest from a technical point of view, enabling opticians to calculate the loss necessarily experienced in various instruments from this source, as well as the intensity where the light is reflected from a single or from two parallel surfaces of glass or other material. Of far more importance, however, is the interest attaching to this subject from a theoretical point of view, and especially in connection with the Undulatory Theory of Light. Formulas for the intensity of the reflected and transmitted beams at all angles have been deduced by eminent supporters of this theory, which in some cases have occasioned considerable discussion. The following very simple formula for the intensity

of common light reflected at a perpendicular incidence was first given by Thomas Young; $I = \frac{(n-1)^2}{(n+1)^2}$; the intensity of the incident beam being equal to unity, and n being the index of refraction of the reflecting substance. The same formula was afterwards reached by Poisson in a rigid and learned analysis of the subject, and it was again deduced by Fresnel.* As is well known Fresnel's formulas were subsequently modified by the celebrated Cauchy, but in Cauchy's formula for reflection as soon as the incidence of the light differs considerably from the polarizing angle, the small quantities which depend on e, the coefficient of ellipticity, become so much reduced that they can be omitted from the numerator and denominator, and Cauchy's formula becomes identical with Fresnel's.† It hence appears that the formula above quoted is as well theoretically established, as any which has been deduced under the guidance of the Undulatory Theory, and one would naturally suppose that it had been often tested carefully by experiment. This does not seem to have been the case, and I do not know that it has ever been rigidly tried by a delicate photometric method. On this account I have made a series of observations on plates of colorless glass which are detailed below.

Mode of experimenting.

When a beam of light falls on a plate of glass or other transparent colorless substance, a certain portion will be reflected, another portion transmitted, a third absorbed. If the plate of glass be colorless and thin, the portion absorbed will be smaller than the necessary errors of observation, so that it can safely be neglected. For example, Bunsen found that in using a plate of crown glass 4·7 millimeters thick, that it absorbed only $\frac{4}{1\,6}$ of one per cent of the *chemical* rays that fell on it at a perpendicular incidence.‡ The thickest plate of glass employed by me was 1·67 millimeters from surface to surface, i. e. about one-third of that used by Bunsen, and as we know that the chemical rays are extinguished by glass in a far larger proportion than those which are luminous, it follows that in the plates mentioned below we can safely neglect the internal extinction. This point being settled, the mode of proceeding becomes quite simple; it is only necessary to measure the amount of light transmitted, and the difference between the incident and transmitted light gives the amount of that reflected, and after making a correction for internal reflection, we shall have the means of comparing the results of theory and experiment.

* Pogg. Annalen. Bd. xxii, p. 98. † Jamin in Pogg. Erg., Bd. iii, p. 256.
‡ Pogg. Annalen, Bd. ci, p. 241.

Mode of determining the indices of refraction.

In my experiments on transparent substances, always where it was possible prisms of the substance were ground and the index of refraction of the sodium line determined as usual with a graduated circle, collimating and observing telescope. In the particular experiments detailed at the termination of this article, the plates of glass used were so thin that it was not practicable to grind from them prisms, and for all such cases I contrived, tested and used two somewhat new modes of procedure, as neither the method of the Duke de Chaulnes (alteration of the focus of a microscope,) nor that of Bernard (displacement of an image viewed obliquely through a plate,) were here found to give reliable results.

1. A minute angular fragment of the glass to be experimented on was placed in a cell, on a glass slide, like those used for mounting microscopic objects, and surrounded with a mixture of "body-sperm-oil" and oil of cassia, the proportions being varied till the refraction of the glass for the sodium line had been exactly compensated by the oil. *Olive oil* became turbid when mixed with the oil of cassia and hence could not be used. The mode of comparing the refractive power of the mixture of the oils and glass was as follows: at the distance of half an inch below the level of a microscope stage was a fine slit, cut in tinfoil which had been pasted on glass; the microscope was focussed on this, a sodium flame being used to illuminate it; the cell with the oil and fragment of glass was then placed on the stage of the microscope, and moved so that the light from the slit passed through the angular fragment, when it would happen that the line of light would be refracted to the right or left hand according as oil or glass predominated in refractive power, which made it instantly evident whether sperm oil or oil of cassia was needed. A number of experiments were made to test this method, which was found to answer well, the index of refraction as determined by prism corresponding with that obtained by the use of the fragment: so in the case of a sample of crown glass a triangular prism gave the index of refraction as 1·526, while by the new method it was found to be 1·529. This method, however, is capable of still more accurate results, as in the above mentioned experiments, the compensation was pushed only far enough to answer my immediate purpose; that is to say, the fragment may be considered to consist of one *large* triangular prism with a moderate angle, and a number of smaller prisms, some of which are sure to have very large angles; these latter become effective when the glass is under oil, total reflection no longer taking place, and they act powerfully on the light coming from the slit, still furnishing faint images having

a considerable deviation even after the main portion of the glass fragment has ceased to perceptibly deflect it.

In no case did I push the compensation far enough to gather in all these outstanding beams.

2. Another method, which in the case of crown glass was found to answer quite well enough for my purpose and to be very convenient, consisted in fusing to a spherical globule a fragment of the glass, placing it in the mixed oils and effecting compensation by observing when the globule ceased to act as a lens, for which purpose a small telescope or the microscope can be employed. Thus, for example, a certain kind of crown glass when ground into a prism gave as index of refraction for the sodium line 1·526, while when tested according to this second method, the results of two experiments were 1·5232 and 1·5235.

I give now the results of careful sets of experiments on the amount of light transmitted by two different samples of crown glass: in each case the results of four independent trials are given, each trial being worked out with the aid of seven double compensations. In the first case the thickness of the glass was ·15 millimeters, the index of refraction 1·5236, and allowing for the effects of internal reflection, it should, according to theory, have transmitted 91·736 per cent of the light falling on it. Experiment gave:

92·227
91·371
91·019
91·143
91·440

The difference ·296 being hardly larger than the necessary error connected with the method of making the determination.

In the second case the index of refraction was 1·5225, the thickness 1·677 millimeters, and by theory it should have transmitted 91·763 per cent of the light falling on it. Below are the actual results obtained:

90·886
90·948
90·892
91·895
91·155

The difference here of ·500 per cent, or $\frac{1}{200}$ of the whole amount, is almost equally satisfactory, and these experiments show, I think, that the reflecting power of glass with the above index of refraction, *conforms in the closest manner to the predictions of theory.* Elaborate experiments were also made with flint glass, quartz and calc-spar, but I suppress the results, as it after-

wards turned out that they were contaminated with minute errors of the character described in this article under the head "mode of adjusting, &c." 3rd; the adjustment alluded to having indeed been always made, but not with a sufficient degree of care to exclude the last trace of error.

New York, March 29th, 1870.

ART. II.—*The Ethers of Arsenic Acid and of Arsenious Acid;* by J. M. CRAFTS.*

MR. FRIEDEL and I observed, while studying the ethers of silicic acid, that the silicate of ethyl is readily decomposed by heating it with anhydrous boracic acid in a sealed tube; that pure borate of ethyl is formed, and that silicic acid, or an ether containing a very large proportion of silicic acid, is deposited in the tube. It occurred to me that a similar reaction might offer a convenient method of preparing the ethers of feeble acids. The ethers of the acids of arsenic and antimony and of tungstic acid have not yet been obtained, and experiments were made with a view to their preparation, but I have only succeeded in the case of arsenious acid in obtaining a new ether by the action of the acid upon silicic ether. Another method, however, afforded the means of preparing the ethers of arsenic acid, and several different methods of preparing the ethers of arsenious acid have been discovered. The present paper is devoted to the description of the methylic, ethylic and amylic ethers of both acids of arsenic.

ARSENIATE OF ETHYL.

The first attempts were made to prepare this body by the action of arsenic acid on the silicate of ethyl. The arsenic acid was dried by heating it in a current of dry air, and it was then sealed in a glass tube with the silicate of ethyl, and heated in an air-bath. After heating 10 hours at 210° centigrade there appeared to be no reaction; on heating 3 hours longer at 230°, a considerable quantity of the arsenic acid dissolved and at a little higher temperature the tube exploded, probably in consequence of the oxydation of the ether by the arsenic acid.

In another experiment 20 grams of silicate of ethyl were heated with 8 grams of arsenic acid for 6 hours at 220°–230°. A gelatinous silicate of ethyl was deposited, and on opening the tube about ½ litre of a gas having the properties of ethylene was evolved. A considerable quantity of common ether was formed, and the remainder of the liquid contents of the tube was par-

* The chemical symbols used have the values which belong to them in the new system.

tially decomposed on distillation. After several distillations a small quantity of a body was isolated, which contained arsenious, but no arsenic acid, and which proved to be an impure arsenious ether. It is evident from these results that, at the temperature required for the action of arsenic acid upon the silicate of ethyl, the greater part of the product is decomposed with formation of common ether and ethylene, while a small quantity of the arsenic acid is reduced and arsenite of ethyl is formed, and that the reaction does not furnish any arseniate of ethyl.

The arseniate of ethyl can be easily prepared by the action of the iodid of ethyl on the arseniate of silver—

$$3C_2H_5I+Ag_3AsO_4=3AgI+(C_2H_5)_3AsO_4.$$

When the two bodies, mixed with pure anhydrous ether are heated together at about 90° in a sealed tube, the reaction is completed in a few hours, and the point at which this takes place is marked by the change of color of the silver salt from red to light yellow. It is important that the arseniate of silver should be in excess and that the temperature should not exceed 100°, as the iodid of ethyl decomposes a portion of the arseniate of ethyl, and the decomposition takes place more readily, the larger the quantity of iodid and the higher the temperature.

The liquid together with the iodid of silver is taken from the tube and washed with pure ether until it is entirely freed from arseniate of ethyl. This operation can be most conveniently performed on a funnel, provided with a glass plate ground to fit it. A hole should be drilled in the plate, and a funnel tube ground into it, in order that the liquid may be poured upon the filter without removing the glass plate.

The solution in ether is distilled and finally heated in a water-bath, while a current of dry air is passed through it to carry off the last trace of ether. The arseniate of ethyl thus obtained can be most easily purified by distillation in a partial vacuum, as it is decomposed in part by distillation in the air. In operations of this kind it is preferable to distil under a diminished pressure rather than to push the exhaustion of the air as far as possible since the boiling point remains more constant, when the pressure is high. This probably is due in great part to the fact, that a slight variation of pressure does not alter the relative pressure so much, when the exhaustion is incomplete, as when it is nearly complete. For instance, the pressure may well vary 2 millimeters during the course of a distillation, and a variation from 5 to 7 millimeters affects the boiling point much more than a variation from 200 to 202 millimeters.

In the case of the arseniate of ethyl it was found that it distilled under a pressure of 60 millimeters at about 85° lower than in the air, and that this difference of temperature was sufficient

to prevent decomposition. The distilling apparatus was provided with an air reservoir and with a mercury guage, and the pressure was kept nearly constant at 60 millimeters during the course of the distillation.

Arseniate of ethyl distils at 148°–150° under a pressure of 60 millimeters.

About $\frac{4}{5}$ of the theoretical quantity are obtained by the above method of preparation.

The arsenic ethers are immediately decomposed by smallest traces of water and it is essential that the common ether used in their preparation should be absolutely anhydrous. It is also important for the operation in the sealed tube, that it should contain no alcohol, since the iodid of ethyl acts upon alcohol with formation of ordinary ether and water. Ether can be most easily freed from small quantities of water and alcohol by digesting it several times with shavings of sodium, which can be cut with a knife from a large piece, allowing it to stand each time 24 hours with the sodium, and then distilling. Ether so prepared can be heated with sodium to 90° in a sealed tube without the smallest evolution of hydrogen.

The following analyses were made of arseniate of ethyl distilling at 148°–150° under a pressure of 60 millimeters.

I. *Substance* = 0·2980 *grms.*; CO_2 = 0·3470 *grms.*; H_2O = 0·1740 *grms.*

II. *Substance* = 0·9260 *grms.*; *arseniate of magnesium and ammonium* = 0·7705 *grms.*

The determination of arsenic was made by decomposing a weighed quantity of the arseniate of ethyl by water, evaporating with nitric acid, and determining the arsenic acid as the arseniate of magnesium and ammonium on a weighed filter.

	I.	II.	Calculated for $(C_2H_5)_3AsO_4$
C	=31·79	....	31·86
H	= 6·49	....	6·64
As	=....	32·85	33·18

The density of liquid arseniate of ethyl at 0° compared with that of water at 4° = 1·3264. Its density at 8·8° = 1·3161.

The density of vapor of arseniate of ethyl can not be taken on account of its decomposition by heat. This decomposition, however, is not so great, but that its boiling point in the air can be determined with sufficient accuracy. When a portion of the arseniate of ethyl, which has been purified by distillation under a diminished pressure, is distilled in the air, the greater part of it passes at 235°–238°, but toward the last part of the distillation decomposition ensues, and a white frothy deposit of arsenic acid is left in the retort.

Arseniate of ethyl attracts moisture from the air and is immediately decomposed with formation of arsenic acid and al-

cohol. It dissolves immediately in water, undergoing the same decomposition. Particular attention was paid to the question of the existence of ethyl-arseniates, and experiments were made under the most favorable conditions for observing their formation, but thus far with negative results. Felix D'Arcet, (Ann. Chem. und Pharm., xix, p. 202), gives the analysis of the barium salt of the di-ethyl arsenic acid $Ba(C_2H_5)_4(AsO_4)_2$; but Hugo Schiff (Ann. Chem. und Pharm., cxi, p. 370,) failed to obtain this body on dissolving arsenic acid in alcohol, evaporating until the solution acquired the consistency of a syrup, dissolving in water and neutralizing with baric carbonate. This result proves at least that no ethyl arsenic acid can exist in aqueous solution, and my own experiments confirm those of Mr. Schiff. It appeared possible that when the arseniate of ethyl is decomposed by water, the decomposition might pass through two stages, and that ethyl arsenic acids might at least exist for a time in the aqueous solution, even though they were ultimately completely destroyed. In order to test this idea, a weighed quantity of arseniate of ethyl was dissolved in water, litmus solution was added and it was neutralized immediately with a standard solution of ammonia. If the decomposition with water had been a gradual one with formation of the acids mentioned, the neutralized solution should have developed an acid reaction after the lapse of a considerable time. This was not the case. The same amount of ammonia was required for neutralization, either immediately, or after the solution had stood 24 hours, and moreover, the reaction was exactly similar to that of a corresponding quantity of pure arsenic acid dissolved in water. It should be noticed that no sharp change of color can be obtained by neutralizing a solution of arsenic acid containing litmus with ammonia.

Another attempt was made to obtain the ethyl arsenic acid in an alcoholic solution. Pure arseniate of ethyl was mixed with alcohol, and only enough aqueous alcohol was added to furnish water for the decomposition expressed by the reaction:

$$(C_2H_5)_3\,AsO_4+H_2O=H(C_2H_5)_2\,AsO_4+C_2H_6O;$$

and the mixture was distilled in vacuo. It behaved exactly like a solution of arsenic acid in arseniate of ethyl: the latter distilling and leaving the anhydrous arsenic acid as a residue. It was thought probable that in this experiment the first product of the action of water would enter into the following reaction with some of the undecomposed arseniate of ethyl:

$$H(C_2H_5)_2\,AsO_4+(C_2H_5)_3\,AsO_4=(C_2H_5)_4As_2O_7+C_2H_6O;$$

but no condensed ether like the one whose formula is given was obtained, and the only result of the decomposition with aqueous alcohol under these circumstances is the setting free of arsenic acid and alcohol.

Arseniate of ethyl is immediately decomposed by ammonia with the formation of a crystalline body, which it is very difficult to obtain pure on account of the extreme avidity with which it absorbs moisture. The results of several analyses lead to the conclusion, that the body which is first formed has the composition: $(C_2H_5)_2NH_2AsO_3$, and that it absorbs water to form a di-ethyl arseniate of ammonium, $(C_2H_5)_2NH_4AsO_4$, but the study of this product is not yet completed.

Arseniate of Methyl.

The arseniate of methyl can be prepared from the iodid of methyl and arseniate of silver in the same way that the arseniate of ethyl is prepared. The crude product is purified by distillation under a pressure of sixty millimeters. At this pressure it distils without decomposition at 128°–130°.

I. *Substance*=0·4500 *grms.*; CO_2=0·3285 *grms.*; H_2O=0·1980 *grms.*

II. *Substance*=0·8070 *grms.*; As_2O_5=0·4966 *grms.*

The determination of arsenic was made by decomposing the ether with water, evaporating with nitric acid, and heating the arsenic acid thus formed with oxyd of lead.

	I.	II.	Calculated for $(CH_3)_3AsO_4$.
C	=19·90	----	19·57
H	= 4·91	----	4·89
As=		40·15	40·76

The density of liquid of arseniate of methyl at 14°·5, compared with water at 4°, =1·5591.

When pure arseniate of methyl is distilled in the air, the greater part passes at 213°–215°, but there is a partial decomposition with separation of arsenic acid.

It resembles the arseniate of ethyl in all its properties. It is a colorless liquid, miscible in all proportions with water, and it is decomposed immediately into arsenic acid and alcohol by the action of water or moisture for the air.

Arseniate of Amyl.

This body is produced by action of iodid of amyl on the arseniate of silver, but it cannot be obtained in a state of purity, because it decomposes, even when distilled in vacuo.

The crude product of the reaction, after having been heated to 170°, was mixed with pure ether, when a considerable quantity of arsenious acid was precipitated; after filtering, the solution was heated in an oil-bath, and finally a current of dry air was passed through it, while the bath was kept at 200°. An analysis of the product so obtained gave C=39·31 per cent and H=7·64 per cent, instead of C=51·14 per cent and H=9·37 per cent.

A somewhat better result was obtained by distilling the crude product in vacuo. The greater part is decomposed at about 200°, but a small quantity of ether distils unchanged. Some of this distillate was heated to 195° in current of dry air, in order to free it as far as possible from products of decomposition. The analysis of this product gave C=44·82 per cent and H=8·41 per cent instead of C=51·14 per cent and H=9·37 per cent.

The products analyzed appear to have been arseniate of amyl mixed with arsenic acid.

Arsenite of Ethyl.

The ethers of arsenious acid can be prepared more readily than these of arsenic acid, and several reactions may be employed to obtain them.

Arsenite of ethyl is produced in the following reactions:

By the action of arsenious acid on the silicate of ethyl. By the action of iodid of ethyl on the arsenite of silver. By the action of the chlorid or bromid of arsenic on the alcoholate of sodium. The iodid of arsenic and the alcoholate of sodium give no arsenite of ethyl.

When arsenious acid is heated with alcohol to about 200° in a sealed tube, a considerable quantity of the acid dissolves, and crystallizes out on cooling as beautifully formed octahedrons. A small quantity of arsenic is reduced and a corresponding quantity of aldehyd is formed. No arsenious ether is formed.

Arsenious acid was heated with a mixture of common ether and acetate of ethyl for 20 hours at 200°, but no reaction took place.

It seemed highly probable that arsenite of ethyl might be obtained by heating together, common ether and chlorid of arsenic according to the reaction

$$AsCl_3+3(C_2H_5)_2O=(C_2H_5)_3AsO_3+3C_2H_5Cl,$$

but this proved not to be the case, for after the two substances had been heated together 20 hours at 200°, it was found that no chlorid of ethyl and no arsenite of ethyl had been formed.

It has been already noticed that by a process of reduction arsenite of ethyl is produced, when arsenic acid is heated with silicic ether. When arsenious acid is heated to about 200° in a sealed tube with silicic ether, the only reaction which takes place is the replacement of the silicic acid by the arsenious acid, a gelatinous deposit of a silicic ether, containing a very large quantity of silicic acid, is formed in the tube; no alcohol, ether or gaseous body is produced, and the arsenious ether can easily be obtained pure by distillation.

Iodid of ethyl acts less readily upon the arsenite of silver than upon the arseniate of silver, and the amount of product

obtained approaches less nearly to the theoretical quantity; there is, however, no difficulty in obtaining pure arsenious ether by heating together iodid of ethyl and dry arsenite of silver mixed with pure ether in a sealed tube to 150°. The arsenious ether can be separated by washing the silver salt with common ether and finally by distilling the solution in ether.

The action of alcoholate of sodium upon the chlorid, or, better still, upon the bromid of arsenic, furnishes the best method for obtaining the arsenite of ethyl.

It is well known that the chlorid of arsenic combines with alcohol with evolution of heat to form a definite compound,* which distils without decomposition.

When the bromid of arsenic is mixed with an equal volume of alcohol, the heat evolved is very slight (an elevation of temperature of 3° was noticed), and no definite combination appears to be formed. If alcoholate of sodium is added to a mixture of either the chlorid or bromid of arsenic with alcohol, a precipitation of chlorid or bromid of sodium takes place immediately, and arsenite of ethyl is formed, but if the addition of the alcoholate is continued until all the chlorine or bromine is combined with sodium, the arsenite of ethyl is destroyed at the same time.† If the operation is terminated before all the chlorid or bromid of arsenic has been acted upon, a mixture of the undecomposed chlorid or bromid with arsenite of ethyl is obtained.

It was thought at first that the difference between the boiling point of the bromid of arsenic (222°) and that of the arsenite of ethyl (166°) would suffice for their separation, but this proved not to be the case, for a mixture of the two bodies in any proportion distils like a definite compound, and no separation by fractional distillation is possible. This kind of combination between the chlorid or bromid of arsenic and the ethers of arsenious acid will be noticed below.

The arsenite of ethyl, which is produced in the reaction above described, can only be isolated by taking advantage of its singular property of resisting entirely the action of dry ammonia gas, while the chlorid or bromid of arsenic combines with ammonia to form a crystalline solid insoluble in alcohol and ether. The method which was usually employed to prepare the arsenite of ethyl consists in dissolving 5 parts of bromid of arsenic in absolute alcohol and in adding to the solution the liquid obtained by adding 1 part of sodium to a considerable quantity of alcohol. The reaction is not violent, and it is not necessary to cool the vessel in which it takes place.

* De Luynes, Ann. Chem. und Pharm., cxvi, p. 368.

† See paper in Comptes Rendus, April, 1867.

The arsenite of ethyl and undecomposed bromid of arsenic are separated from the bromid of sodium precipitate by washing with alcohol. The alcoholic solution is then distilled in a water-bath, pure ether is added to the residue, and dry ammonia gas is passed through the solution in ether in order to precipitate the bromid of arsenic, which is mixed with the arsenite of ethyl. The latter is separated from precipitate by washing with ether, the ether is distilled in a water-bath, and finally a current of dry air or carbonic acid is passed through. The residue consists of arsenite of ethyl containing some arsenious acid and bromid of sodium in solution. It can be purified completely by two or three distillations. Pure arsenite of ethyl boils at 165°–166° (no correction).

Analyses were made of products obtained by the three methods of preparation described above.

I. *Substance*=0·2320 *grms.*; CO_2=0·2925 *grms.*; H_2O=0·1590 *grms.*

II. *Substance*=0·4960 *grms.*; C_2O=0·6245 *grms.*; H_2O=0·3160 *grms.*

III. *Substance*=0·2510 *grms.*; *arseniate of magnesium and ammonium*=0·2311 *grms.*

The determination of arsenic was made by weighing the substance in a bulb, breaking the bulb under a solution of caustic potash, oxydizing the arsenious acid by chlorate of potash and chlorhydric acid at a temperature of 50°, and precipitating by sulphate of magnesium and chlorid of ammonium in the ordinary way.

I.	II.	III.	Calculated for $(C_2H_5)_3As\,O_3$.
C=34·38	34·33	----	34·29
H= 7·61	7·08	----	7·14
As=	----	36.34	35·71

The density of liquid arsenite of ethyl compared with that of water at 4°=1·224.

Several vapor density determinations were made at different temperatures by Dumas' method.

Difference between the weights of the bulb=0·9610 *grms.*
Temperature of the balance, = 4·°2
Temperature of the oil-bath (*air thermometer*) =267·°0
Capacity of the bulb, =269·5 *c. c.*
Air remaining, = 0·5 *c. c.*

Vapor density=7·389 Theory=7·2673

The vapor density at 209·°5=7·615
" " 213° =7·608
" " 233° =7·197

The last determination is probably too small through some fault of manipulation. It is to be expected that the numbers should be a little too high, since the arsenite of ethyl is decomposed by the slightest trace of moisture, leaving a deposit of arsenious acid in the bulb. The arsenite of ethyl is a colorless liquid with a peculiar, but not disagreeable, odor, and sufficiently volatile to make its presence plainly perceptible in a room, where it is standing in an open test-tube.

The ethers of arsenic do not appear to have the extraordinarily poisonous properties, which are attributed to arseniuretted hydrogen, at least I have suffered no inconvenience after having worked with them for several years.

Arsenite of ethyl is miscible in any proportion with alcohol and ether, but it is decomposed by aqueous alcohol. The only products of decomposition are arsenious acid and alcohol, and no compound ether with the formula, $(C_2H_5)_4As_2O_5$, is formed. This ether is also not produced according to the reaction:

$$4(C_2H_5)_3\,AsO_3+As_2O_3=3(C_2H_5)_4As_2O_5$$

when arsenite of ethyl is heated to 300° with arsenious acid. Arsenite of ethyl at its boiling point dissolves about $\frac{1}{3}$ its weight of arsenious acid, but a considerable quantity separates out, when the solution is allowed to become cool. Arsenite of ethyl can be distilled unchanged over dry arsenious acid.

The reaction between the arsenite of ethyl and bromhydric acid is the reverse of that which is usual between bromhydric acid and the ethers of acids. Usually the bromid of the alcoholic radical is formed, and the acid is set free. In this case the reaction leads to the formation of alcohol and bromid of arsenic, and it may be expressed by the equation:

$$(C_2H_5)_3AsO_3+3HBr=3C_2H_6O+As\,Br_3.$$

All that is required to study this reaction is to pass dry bromhydric acid through arsenite of ethyl, when the alcohol can be separated by distillation, leaving a crystalline residue of bromid of arsenic.

It was thought possible that bromine might combine directly with the arsenite of ethyl to form the compound: $(C_2H_5)_3\,AsO_3Br_2$, but it was found that the bromine substitutes itself for hydrogen in the ethyl, and that the substituted ether thus formed is decomposed by the bromhydric acid which is evolved in its formation.

Iodine does not act upon the arsenite of ethyl, and the two bodies can be separated by distillation. Dry ammonia gas is entirely without action upon the arsenite of ethyl as well as upon the other ethers of arsenious acid, and they can even be distilled in an atmosphere of ammonia without alteration.

Arsenite of Methyl.

The arsenite of methyl was prepared by heating dry arsenious acid with the silicate of methyl for 12 hours at 160°, and very nearly the theoretical quantity was obtained.

It was also obtained by the action of iodid of methyl on arsenite of silver in a sealed tube. The reaction does not take place at a temperature lower than 150°, and at that temperature a considerable part of the arsenious ether is destroyed, so that the first method of preparation is to be preferred.

The arsenite of methyl boils at 128°–129° (no correction).

I. *Substance*=0·4990 *grms.*; Co_2=0·3966 *grms.*; H_2O=0·2442 *grms.*

II. *Substance*=0·3725 *grms*; As_2O_5=0·2525 *grms.*

The arsenic determination was made by decomposing the ether with water, oxydizing with nitric acid, evaporating, and combining the arsenic acid with oxyd of lead.

I.	II.	Calculated for $(CH_3)_3AsO_3$.
C=21·67	----	21·44
H= 5·44	----	5·36
As= ----	44·21	44·64

The density of liquid of arsenite of methyl at 9°·6 compared with that of water at 4°=1·428. The vapor density was determined from the following data:

Difference between weights of bulb	=0·5265 *grms.*
Temperature of the balance,	=16°·4
Temperature of the oil-bath (air-thermometer)	=197°
Capacity of the bulb,	=161·5 *c. c.*
Air remaining,	= 1·0 *c. c.*
Vapor density=6·006	Theory=5·818

The determination probably came out too high on account of the decomposition of a small quantity of ether by the moisture of the air.

The arsenite of methyl resembles the arsenite of ethyl in its properties. It is decomposed immediately by water and also by the moisture of the air with formation of arsenious acid and methylic alcohol.

In endeavoring to obtain the chlorhydrines of these ethers a singular class of compounds in indefinite proportions, but with constant boiling points, was discovered. The composition of the chlorhydrines of the arsenite of methyl would be represented by the formulas: $(CH_3)_2ClAsO_2$; CH_3Cl_2AsO; and in order to obtain them, arsenious acid was heated with the chlorhydrines of the silicate of methyl.* In this way bodies can be easily obtained, having the required composition and con-

* This Journal, II, xliii, p. 155 and 331.

stant boiling points, but bodies with their constituents in every intermediate proportion between these, whose formulas are given above, can also be obtained, and they have equally constant boiling points. In fact, by combining the chlorid of arsenic and an arsenious ether in indefinite proportions, a body with a constant boiling point can be obtained; and what renders this fact most remarkable, is that the boiling point is not a mean between those of the two bodies mixed, but it is higher than either of them. The boiling point of arsenite of methyl is 129° (no correction). The boiling point of chlorid of arsenic is 133° (no correction). A product containing 42 per cent of chlorid of arsenic, and 58 per cent of arsenite of ethyl distilled unchanged at 143°–144°. A product containing 11·5 per cent of chlorid of arsenic and 88·5 per cent of arsenite of ethyl distilled at 131°–132°. I am not aware that an analogous case of combination in indefinite proportions has ever been observed.

The boiling point of the arsenite of ethyl is 166° (no correction). Two parts of arsenite of ethyl and one part of chlorid of arsenic mix with slight disengagement of heat, and the product distils constantly at 159°–164° (no correction).

A mixture of equal parts of chlorid of arsenic and of arsenite of ethyl distils at 150°–152°.

A body was also obtained by the action of alcoholate of sodium upon the bromid of arsenic in excess, which contained 65·75 per cent of bromid of arsenic and 34·25 per cent of arsenite of ethyl, and which distilled constantly at 182°–185° (no correction). The chlorid or bromid of arsenic in all these bodies combines directly with dry ammonia gas, leaving the arsenious ether free.

Arsenite of Amyl.

The arsenite of amyl was prepared by the action of the amylate of sodium upon the bromid of arsenic in the same way as was described for the preparation of the arsenite of ethyl.

The product, however, cannot be purified by distillation in the air, since it decomposes in part at its boiling point. It can, however, be purified by distillation under a pressure of 60 millimeters. The following analysis was made of a product boiling at 193°–194° (no correction) under a pressure of 60 millimeters.

Substance=0·4090 *grms.*; CO_2=0·7950 *grms.*; H_2O=0·3578 *grms.*

	Calculated for $(C^5H_{11})_3AsO_3$.
C=53·01	53·57
H= 9·72	9·82

The density of liquid arsenite of amyl at 0° compared with that of water at 4°=1·0525. Arsenite of amyl, which has been purified by distillation under a diminished pressure, distils in the air at 288°–290°, but toward the last part of the distillation arsenious acid separates out.

Arsenite of amyl is not more stable than the other ethers of arsenious acid. It is immediately decomposed by water and also by the moisture of the air.

Arsenite of amyl has analogous properties with the arsenites of ethyl and methyl.

The relations between the boiling points of the bodies described in this paper present some curious anomalies, and do not fall within Kopp's law derived from the study of the more simple ethers.

Boiling points under a pressure of 60 millemeters:

$(C_2H_5)_3AsO_4$=149°
$(CH_3)_3AsO_4$ =129°

Difference = 20°

Boiling points under the atmospheric pressure:

$(C_2H_5)_3AsO_3$=166°
$(CH_3)_3AsO_3$ =129°

Difference = 37°

In the first case the difference of boiling point for each increment of CH_2 is $6\frac{2}{3}$°.

In the second case the difference is $12\frac{1}{3}$°.

I have already stated in a communication to the French Academy of Sciences, (April 2, 1867), that tungstic and antimonius acids give no ethers when they are heated with the silicate of ethyl.

ART. III.—*On the probable seat of Volcanic Action;* by T. STERRY HUNT, LL.D., F.R.S.*

THE igneous theory of the earth's crust, which supposes it to have been at one time a fused mass, and to still retain in its interior a great degree of heat, is now generally admitted. In order to explain the origin of eruptive rocks, the phenomena of volcanos, and the movements of the earth's crust, all of which are conceived by geologists to depend upon the internal heat of the earth, three principal hypotheses have been put forward. Of these the first supposes that in the cooling of the globe a solid

* From the Geological Magazine for June, 1869.

crust of no great thickness was formed, which rests upon the still uncongealed nucleus. The second hypothesis, maintained by Hopkins and by Poulett Scrope, supposes solidification to have commenced at the center of the liquid globe, and to have advanced toward the circumference. Before the last portions became solidified, there was produced, it is conceived, a condition of imperfect liquidity, preventing the sinking of the cooled and heavier particles, and giving rise to a superficial crust, from which solidification would proceed downward. There would thus be enclosed, between the inner and outer solid parts, a portion of uncongealed matter, which, according to Hopkins, may be supposed still to retain its liquid condition, and to be the seat of volcanic action, whether existing in isolated reservoirs or subterranean lakes; or whether, as suggested by Scrope, forming a continuous sheet surrounding the solid nucleus, whose existence is thus conciliated with the evident facts of a flexible crust, and of liquid ignited matters beneath.

Hopkins, in the discussion of this question, insisted upon the fact established by his experiments, that pressure favors the solidification of matters which, like rocks, pass in melting to a less dense condition, and hence concludes that the pressure existing at great depths must have induced solidification of the molten mass at a temperature at which, under a less pressure, it would have remained liquid. Mr. Scrope has followed this up by the ingenious suggestion that the great pressure upon parts of the solid igneous mass may become relaxed from the effect of local movements of the earth's crust, causing portions of the solidified matter to pass immediately into the liquid state, thus giving rise to eruptive rocks in regions where all before was solid.*

Similar views have been put forward in a note by Rev. O. Fisher, and in an essay on the formation of mountain chains, by Mr. N. S. Shaler, in the proceedings of the Boston Society of Natural History, both of which appear in the *Geological Magazine* for November last. As summed up by Mr. Shaler, the second hypothesis supposes that the earth "consists of an immense solid nucleus, a hardened outer crust, and an intermediate region of comparatively slight depth, in an imperfect state of igneous fusion." In this connection it is curious to remark that, as pointed out by Mr. J. Clifton Ward, in the same Magazine for December (page 581), Halley was led, from the study of terrestrial magnetism, to a similar hypothesis. He supposed the existence of two magnetic poles situated in the earth's outer crust, and two others in an interior mass, separated from the solid envelope by a fluid medium, and revolving, by a very

* See Scrope on Volcanos, and his communication to the *Geological Magazine* for Dec., 1868.

small degree, slower than the outer crust.* The same conclusion was subsequently adopted by Hansteen.

The formation of a solid layer at the surface of the viscid and nearly congealed mass of the cooling globe, as supposed by the advocates of the second hypothesis, is readily admissible. That this process should commence when the remaining envelope of liquid was yet so deep that the refrigeration from that time to the present has not been sufficient for its entire solidification, is, however, not so probable. Such a crust on the cooling superficial layer would, from the contraction consequent on the further refrigeration of the liquid stratum beneath, become more or less depressed and corrugated, so that there would probably result, as I have elsewhere said, "an irregular diversified surface from the contraction of the congealing mass, which at last formed a liquid bath of no great depth, surrounding the solid nucleus." Geological phenomena do not, however, in my opinion, afford any evidence of the existence of yet unsolidified portions of the originally liquid material, but are more simply explained by the third hypothesis. This, like the last, supposes the existence of a solid nucleus, and of an outer crust, with an interposed layer of partially fluid matter, which is not, however, a still unsolidified portion of the once liquid globe, but consists of the outer part of the congealed primitive mass, disintegrated and modified by chemical and mechanical agencies, impregnated with water, and in a state of igneo-aqueous fusion.

The history of this view forms an interesting chapter in geology. As remarked by Humboldt, a notion that volcanic phenomena have their seat in the sedimentary formations, and are dependent on the combustion of organic substances, belongs to the infancy of geology. To this period belong the theories of Lémery and Breislak (*Cosmos*, v, 443; Otte's translation). Keferstein in his *Naturgeschichte des Erdkörpers*, published in 1834, maintained that all crystalline non-stratified rocks, from granite to lava, are products of the transformation of sedimentary strata, in part very recent, and that there is no well-defined line to be drawn between Neptunian and volcanic rocks, since they pass into each other. Volcanic phenomena, according to him, have their origin not in an igneous fluid center, nor in an oxydizing metallic nucleus (Davy, Daubeny), but in known sedimentary formations, where they are the result of a peculiar kind of fermentation, which crystallizes and arranges in new

* The elevated temperature of the interior of the globe would probably offer no obstacle to the development of magnetism. In a recent experiment of M. Trève, communicated by M. Faye to the French Academy of Sciences, it was found that molten cast iron, when poured into a mould, surrounded by a helix which was traversed by an electric current, became a strong magnet when liquid at a temperature of 1300° C., and retained its magnetism while cooling (Comptes Rendus de l'Acad. des Sciences, Feb., 1869.

forms the elements of the sedimentary strata, with an evolution of heat as a result of the chemical process (*Naturgeschichte*, vol. i, p. 109; also *Bull. Soc. Geol. de France* [1], vol. vii, p. 197). In commenting upon these views (*Am. Jour. Science*, July, 1860), I have remarked that, by ignoring the incandescent nucleus as a source of heat, Keferstein has excluded the true exciting cause of the chemical changes which take place in the buried sediments. The notion of a subterranean combustion or fermentation, as a source of heat, is to be rejected as irrational.

A view identical with that of Keferstein, as to the seat of volcanic phenomena, was soon after put forth by Sir John Herschel, in a letter to Sir Charles Lyell, in 1836 (*Proc. Geol. Soc. London*, ii, 548). Starting from the suggestion of Scrope and Babbage, that the isothermal horizons in the earth's crust must rise as a consequence of the accumulation of sediments, he insisted that deeply buried strata will thus become crystallized by heat, and may eventually, with their included water, be raised to the melting point, by which process gases would be generated, and earthquakes and volcanic eruptions follow. At the same time the mechanical disturbance of the equilibrium of pressure, consequent upon a transfer of sediments, while the yielding surface reposes on matters partly liquified, will explain the movements of elevation and subsidence of the earth's crust. Herschel was probably ignorant of the extent to which his views had been anticipated by Keferstein; and the suggestions of the one and the other seemed to have passed unnoticed by geologists until, in March 1858, I reproduced them in a paper read before the Canadian Institute (Toronto,) being at that time acquainted with Herschel's letter, but not having met with the writings of Keferstein. I there considered the reactions which would take place under the influence of a high temperature in sediments permeated with water, and containing, besides siliceous and aluminous matters, carbonates, chlorids, and carbonaceous substances. From these, it was shown, might be produced all the gaseous emanations of volcanic districts, while from aqueo-igneous fusion of the various admixtures might result the great variety of eruptive rocks. To quote the words of my paper just referred to: "We conceive that the earth's solid crust of anhydrous and primitive igneous rock is everywhere deeply concealed beneath its own ruins, which form a great mass of sedimentary strata, permeated by water. As heat from beneath invades these sediments, it produces in them that change which constitutes normal metamorphism. These rocks, at a sufficient depth, are necessarily in a state of igneo-aqueous fusion; and in the event of fracture in the overlying strata, may rise among them, taking the form of eruptive rocks. When the nature of the sediments is such as generate great amounts of elastic

fluids by their fusion, earthquakes and volcanic eruptions may result, and these—other things being equal—will be most likely to occur under the more recent formations." (*Canadian Journal*, May, 1858, vol. iii, p. 207).

The same views are insisted upon in a paper "On some Points in Chemical Geology" (Quart. Jour. Geol. Soc. London, Nov. 1859, vol. xv, page 594), and have since been repeatedly put forward by me, with farther explanations as to what I have designated above, *the ruins of the crust of anhydrous and primitive igneous rock.* This, it is conceived, must, by contraction in cooling, have become porous and permeable, for a considerable depth, to the waters afterwards precipitated upon its surface. In this way it was prepared alike for mechanical disintegration, and for the chemical action of the acids, which, as shown in the two papers just referred to, must have been present in the air and the waters of the time. It is, moreover, not improbable that a yet unsolidified sheet of molten matter may then have existed beneath the earth's crust, and may have intervened in the volcanic phenomena of that early period, contributing, by its extravasation, to swell the vast amount of mineral matter then brought within aqueous and atmospheric influences. The earth, air, and water thus made to react upon each other, constitute the first matter from which, by mechanical and chemical transformations, the whole mineral world known to us has been produced.

It is the lower portions of this great disintegrated and water-impregnated mass which form, according to the present hypothesis, the semi-liquid layer supposed to intervene between the outer solid crust and the inner solid and anhydrous nucleus. In order to obtain a correct notion of the condition of this mass, both in earlier and later times, two points must be especially considered, the relation of temperature to depth, and that of solubility to pressure. It being conceded that the increase of temperature in descending in the earth's crust is due to the transmission and escape of heat from the interior, Mr. Hopkins showed mathematically that there exists a constant proportion between the effect of internal heat at the surface and the rate at which the temperature increases in descending. Thus, at the present time, while the mean temperature at the earth's surface is augmented only about one-twentieth of a degree Fahrenheit, by the escape of heat from below, the increase is to be found to be equal to about one degree for each sixty feet in depth. If, however, we go back to a period in the history of our globe when the heat passing upward through its crust was sufficient to raise the superficial temperature twenty times as much as at present, that is to say, one degree of Fahrenheit, the augmentation of heat in descending would be twenty times as

great as now, or one degree for each three feet in depth (Geol. Journal, viii, 59.) The conclusion is inevitable that a condition of things must have existed during long periods in the history of the cooling globe, when the accumulation of comparatively thin layers of sediment would have been sufficient to give rise to all the phenomena of metamorphism, vulcanicity, and movements of the crust, whose origin Herschel has so well explained.

Coming, in the next place, to consider the influence of pressure upon the buried materials derived from the mechanical and chemical disintegration of the primitive crust, we find that by the presence of heated water throughout them, they are placed under conditions very unlike those of the original cooling mass. While pressure raises the fusing point of such bodies as expand in passing into the liquid state, it depresses that point for those which, like ice, contract in becoming liquid. The same principle extends to that liquefaction which constitutes solution; where, as is with few exceptions the case, the process is attended with condensation or diminution of volume, pressure will, as shown by the experiments of Sorby, augment the solvent power of the liquid.* Under the influence of the elevated temperature and the great pressure which prevail at considerable depths, sediments should, therefore, by the effect of the water which they contain, acquire a certain degree of liquidity, rendering not improbable the suggestion of Scheerer, that the presence of five or ten per cent of water may suffice, at temperatures approaching redness, to give to a granitic mass a liquidity partaking at once of the character of an igneous and an aqueous fusion. The studies by Mr. Sorby of the cavities in crystals have led him to conclude that the constituents of granitic and trachytic rocks have crystallized in the presence of liquid water, under great pressure, at temperatures not above redness, and consequently very far below that required for simple igneous fusion. The intervention of water in giving liquidity to lavas, has, in fact, long been taught by Scrope, and notwithstanding the opposition of Plutonists, like Durocher, Fournet, and Rivière, is now very generally admitted. In this connection, the reader is referred to the Geological Magazine for February, 1868, page 57, where the history of this question is discussed.

It may here be remarked that if we regard the liquefaction of heated rocks under great pressure, and in presence of water, as a process of solution rather than of fusion, it would follow that diminution of pressure, as supposed by Mr. Scrope, would cause, not liquefaction, but the reverse. The mechanical pressure of great accumulations of sediment is to be regarded as cooperating with heat to augment the solvent action of the water,

* Sorby, Bakerian Lecture, Royal Society, 1863.

and as being thus one of the efficient causes of the liquefaction of deeply buried sedimentary rocks.

[The following extracts from a note by the author to the Geological Magazine for February 1870, may be cited in further elucidation of this point:—"pressure, which in the first case, that of simple fusion of anhydrous materials, prevents liquefaction by preventing expansion, in the second case (that of igneo-aqueous fusion or liquefaction at high temperatures, by the aid of a small portion of water, as maintained by Scrope, Scheerer and Elie de Beaumont) on the contrary, favors liquefaction by promoting the solution of the water-impregnated mass. As Sorby has shown, a conversion of mechanical into chemical force appears in the increase of solubility under pressure. In other words, pressure prevents fusion when, as in most instances, it is a process of expansion, but favors solution, which is, with few exceptions, a process of contraction. Now since I place the seat of volcanic action in a region where solution, rather than simple fusion, is the cause of liquidity, I am led to consider pressure as one of the efficient causes of the liquefaction of rocks, and to regard its diminution as leading to solidification."]*

That the water intervenes not only in the phenomena of volcanic eruptions, but in the crystallization of the minerals of eruptive rocks, which have been formed at temperatures far below that of igneous fusion, is a fact not easily reconciled with either the first or the second hypothesis of volcanic action, but is in perfect accordance with the one here maintained, which is also strongly supported by the study of the chemical composition of igneous rocks. These are generally referred to two great divisions, corresponding to what have been designated the trachytic and pyroxenic types, and to account for their origin, a separation of a liquid igneous mass beneath the earth's crust into two layers of acid and basic silicates, was imagined by Philips, Durocher, and Bunsen. The last mentioned, as is well known, has calculated the normal composition of these supposed trachytic and pyroxenic magmas, and conceives that from them, either separately, or by admixture, the various eruptive rocks are derived; so that the amounts of alumina, lime, magnesia and alkalies, sustain a constant relation to the silica in the rock. If, however, we examine the analyses of the eruptive rocks in Hungary and Armenia, made by Streng, and put forward in support of this view, there will be found such discrepancies between the actual and the calculated results as to throw grave doubts on Bunsen's hypothesis.

* See in this connection, Mr. Scrope on "The Character of Lavas," in the Geological Magazine for March 1870.

Two things become apparent from a study of the chemical nature of eruptive rocks; first, that their composition presents such variations as are irreconcilable with the simple origin generally assigned to them, and second, that it is similar to that of sedimentary rocks, whose history and origin it is, in most cases, not difficult to trace. I have elsewhere pointed out how the natural operation of mechanical and chemical agencies tends to produce among sediments, a separation into two classes, corresponding to the two great divisions above noticed. From the mode of their accumulation, however, great variations must exist in the composition of the sediments, corresponding to many of the varieties presented by eruptive rocks. The careful study of stratified rocks of aqueous origin discloses, in addition to these, the existence of deposits of basic silicates of peculiar types. Some of these are in great part magnesian, others consist of compounds like anorthite and labradorite, highly aluminous basic silicates, in which lime and soda enter, to the almost complete exclusion of magnesia and other bases; while in the masses of pinite or agalmatolite rock we have a similar aluminous silicate, in which lime and magnesia are wanting, and potash is the predominant alkali. In such sediments as these just enumerated we find the representatives of eruptive rocks like peridotite, phonolite, leucitophyre, and similar rocks, which are so many exceptions in the basic group of Bunsen. As, however, they are represented in the sediments of the earth's crust, their appearance as exotic rocks, consequent upon a softening and extravasation of the more easily liquefiable strata of deeply buried formations, is readily and simply explained.*

The object of the present communication has been to call the attention of geologists to the neglected views of Keferstein and Herschel, which I have endeavored to extend and to adapt to the present state of our knowledge. It is proposed in another paper to consider the question of the agencies which have regulated the geographical distribution of volcanic phenomena both in ancient and in modern times.

Montreal, Canada, March, 1869.

* See in this connection the Canadian Journal for 1858, p. 203; Quart. Jour. Geol. Society for 1859, p. 494; this Jour., II, xxxvii. 255, xxxviii, 182; also Geology of Canada, 1863, pp. 643, 669, and Rep. Geol. Canada, 1866, p. 230.

ART. IV.—*Notes on some features of the Flora of Eastern Kansas;* by ELIHU HALL.

[The following consists of two articles (somewhat curtailed) published in the *Prairie Farmer*, which give so clear an exposition of the general features of the vegetation of the region under consideration, that we have sought and obtained permission to reproduce them in this Journal.—EDS.]

TREE GROWTH.—In a trip in September last in a wagon through a portion of Eastern Kansas, from Forts Scott and Humboldt north to Leavenworth and Atchison, I had excellent opportunities for observation of its general flora, and more especially so that of the autumnal months. The general character of its arborescent flora is decidedly adverse to a favorable impression of the adaptation of the country to tree growth. The question arises why all these dwarfed, distorted, abnormally developed specimens everywhere, as compared with the true type of trees, such as we are accustomed to in our own noble denizens of the Mississippi Valley forests. True, on the larger streams, the black alluvial deposit grow some very fine specimens of black walnut, cottonwood and elms, but this area is quite limited.

We should naturally infer from this character of the native arborescent growth of the country that causes had been long existing and were yet in operation to produce so general a result; but since the settlement or since the planting of trees has been commenced there by the inhabitants, everywhere living evidences are springing up directly opposing all such inferences. In some parts of Jefferson county, artificial groves only ten years old, are already 50 feet high, and appearing, through the misty haze of autumn, like natural groves on the prairies of central Illinois. There are evidences, too, that the native growth, that is, the young trees, in all the bushy regions, are making good speed in becoming saplings of proper proportions; this is particularly so of the hickory, (*Carya alba* and *C. porcina.*)

These many facts satisfy us that the causes of the abnormal tree growth here are not ascribable to an uncongenial climate, nor to aridity or sterility of soil, nor to exposure, for the flourishing artificial groves in Jefferson county are upon as high land as there is probably in the state, so near the Missouri river. The growth of fruit trees, apple, peach, pear, and the grape vine, during the past season, has been prodigious.

SPECIES AND DISTRIBUTION OF NATIVE TREES.—The Oak Family is represented by *Quercus obtusiloba* everywhere in the bushy regions, principally as dwarfed low trees and bushes.

Q. macrocarpa is less abundant, but occurs throughout, some specimens of fair proportions. *Q. Prinus*, (var. *humilis*,) every

where in the bushy regions from one foot to forty feet high, fruiting abundantly; the very smallest bushes. The White Oak (*Q. alba*) was not met with. I doubt if there is a specimen in the State, authors of books on Kansas and travelers to the contrary notwithstanding. It is known to be one of the first oaks that fails westward, and probably does not reach the State at any point.

Quercus coccinea was frequently met with, but is not abundant; all dwarfed specimens.

Quercus rubra is more plentiful, principally young trees.

Quercus palustris, in the southern portion of the State is abundant, the principal tree-growth mostly bushes and young trees, and some large old specimens of fair proportions on the bottoms of the larger streams.

The Hickory Family is represented by *Juglans nigra*, on the streams, but not plentiful; a few large specimens.

Carya alba, *C. sulcata*, *C. porcina*, *C. amara* and *C. olivæformis*, (the Pecan;) the latter abounds abundantly in the southern part of the State, but so far as seen, in bushy specimens or young trees of little promise. *Carya alba* and *C. porcina* are the two species springing up thickly in the bushy regions, and are destined soon to afford an abundance of the best of fuel; are already of size for hoop-poles in many places northward. *Ulmus fulva* and *Ulmus Americana* abound on the streams. These are the two commonest elms everywhere westward; the latter principally prevailing.

Populus monilifera, the common cotton-wood, was frequently met with, but is probably not plenty after leaving the Missouri river. Neither of the Aspens, so common in the regions further north, was seen. The maples are rare trees. *Acer sacharinum* was not seen. A few well grown trees of *Negundo aceroides*, (the box elder). The *Diospyros Virginiana*, (Persimmon,) is an abundant shrub southward, and the Pawpaw (*Asimina triloba*) is scarcely less common in that region.

The Herbaceous Flora, as would be expected, partakes much of the character of the high plain flora west of the Missouri river; the country being chiefly prairie: the number of species is small, but individuals aggregate immensely. Of the grasses, Andropogon furcatus, A. scoparius and Sorghum nutans, compose probably 80 per cent. These are the chief hay grasses, and probably the most used by grazing stock in the summer; but the winter forage plant, as I learned from the inhabitants, the one upon which their cattle graze and fatten during the winter months, and from which Kansas has gained her reputation as a country where stock needed feeding a few weeks in the year, is *Sporobolus heterolepis*. This grass only abounds plentifully in certain localities; it affects the moister and flatter portions of the prairie, and is most common southward. It is a

wide-spread species, however, being plentiful in Nebraska, Iowa and Illinois, and, doubtless, in other localities; more or less abundant all over the west and northwest, extending east also to Connecticut. The species may be easily known by its small contracted panicle and round seeds, and especially by its strong odor when bruised. It is inclined to grow in bunches or stools, and where most abundant it nearly occupies the ground.

The relative proportion of species as they occur in the localities mentioned below, will pretty fairly represent the *Graminea* in those regions.

In one hundred square feet on a high prairie in Jefferson county:—Andropogon furcatus, 80 per cent; Andropogon scoparius, 8 per cent; Sorghum nutans, 6 per cent; Kœleria cristata, 4 per cent; Bouteloua curtipendula, 2 per cent.

In the same area in another locality in Jefferson county: Sorghum nutans, formed 20 per cent; Andropogan scoparius, 60 per cent; Sporobolus heterolepis, 8 per cent; Panicum virgatum, 8 per cent; Panicum pauciflorum, 4 per cent.

Same area in Franklin county, high prairie:—Andropogon furcatus, 6 per cent; Andropogon scoparius, 30 per cent; Sorghum nutans, 45 per cent; Bouteloua curtipendula, 10 per cent; Panicum virgatum, 8 per cent; Panicum dichotomum, 1 per cent.

Same area in Anderson county; a thin soil, high prairie:—Andropogon scoparius, 50 per cent; Andropogon furcatus, 2 per cent; Sorghum nutans, 40 per cent; Koeleria cristata, 7 per cent; Panicum pauciflorum, 1 per cent.

These stations are all on the prairie, where no local conditions determine the character of the vegetation, the soils not affected by alkali or a superabundance of lime, but are good agricultural soils. Other stations are occupied with special species. The strongest alkaline places are almost exclusively occupied with *Vilfa cuspidata* and *V. depauperata*, two low-growing species, with very fine leaves thickly set on the ground; probably nearly worthless for grazing purposes. Another peculiar feature of the country consists of the rocky places, so-called. These are generally found in the vicinity of streams on the ground grading from the high prairies. Their peculiarity consists in being everywhere distinctly lined out and occupied abundantly by a very notable *Helianthus*, (sunflower,) *H. orgyalis*, a species peculiar to that region of the country and southward. These localities do not reach the northern portion of the State. These rocky lands furnish the botanist with his richest treasures. Another very notable plant of the southern and middle portions of Kansas, and peculiar to those regions, is *Amphiachyris dracunculoides*. This is an annual, resembling *Solidago linifolia*, but bearing an abundance of showy, yellow flowers in September. It has spread from its native habitat,—the rocky soils,—

to the road sides and into the fields of the farmers, where it has already become a formidable weed. Imagine a field of flax in full bloom, and you have a picture of many fields of the Kansas farmers, only differing in appearance by the flowers being yellow instead of blue. It has different local names, such as Butter-weed, Tumble-weed, etc. A shopkeeper in Humboldt, to whom I applied for information, rather contemptuously replied, "Curious any body didn't know tumble weed." Let not our readers imagine that the plant is *Amaranthus albus*, or *Cycloloma platyphylla*, that so often tumble over the fences and hedges in Illinois in the autumn months. But the Kansas plant does tumble some, too. When not growing too thickly, it forms a rather bushy, round head.

But probably the most notable plant on the prairies, at this season of the year, the most universally distributed, and the most showy, is *Salvia Pitcheri*, a species of sage, three to five feet high, bearing spikes of copious, rather large, blue flowers. These give coloring to the prairie landscape more or less abundantly (except in the northern counties, where it becomes much rarer) as far as the vision extends on either side, towering above the grasses and most other plants, crowning the whole verdant plain below with a canopy of blue. Another species of Sage (*Salvia trichostemoides*) is common also. This is more of a weed in appearance, and even threatens to become troublesome to the agriculturist.

The general herbaceous flora is largely represented with the *Helianthi*. *Helianthus lenticularis*, a showy species, much resembling the common annual sun-flower, is too abundant, being more or less troublesome as a weed to the farmer. *Helianthus petiolaris*, another annual species, is less common. *H. grosseserratus*, *H. Maximiliani* on most rich soils. *H. rigidus* everywhere abounds, and *H. mollis* occurs in all the southern portions in immense quantity, but entirely disappears northward. Only *Silphium laciniatum* was seen. The other notable prairie species of "rosin weed," *Silphium terebinthinaceum* does not probably occur. These plants I find are looked after by land hunters as indications of a good soil. I find the one species here of all heights, from a foot to six feet; the general average is about three feet this wet season, which is much below the ordinary stature of the plant as seen on the prairies of Illinois. Some other Compositæ are largely represented: *Aster multiflorus*, *Solidago rigidus*, *S. linifolius*, and *S. Missouriensis* being exceedingly abundant in specimens. The *Vernonia fascicularis* that gives so much character and notability to the bottom lands and wet prairies of Illinois in the autumn months, is here represented by an allied species, *Vernonia Arkansana*, but in much fewer specimens.

Lespedeza capitata, of the Pea family, is everywhere abundant on the prairies, and cut with the hay often forms too large a portion of tonnage for the interest of the buyer; which also must be said of *Liatris scariosa* and *L. squarrosa*; the former sometimes equal to a fourth or fifth of the gross weight of a stack. Two other species, but less abundant, are represented in the State, *L. punctata* and *L. pycnostachya*. These noble plants empurple the landscape on thousands of acres in August and September. *Gerardia asperifolia* is abundant almost everywhere on the prairie, and contributes a good share to the weedy character of the hay. *Eryngium Leavenworthii* in the extreme south is a showy, handsome plant, and, though a biennial, well deserves a place in ornamental grounds. *Mamillaria vivipara* and a form of *Opuntia Missouriensis* represent the *Cactaceæ*, so far as seen.

WEEDS.—The Kansas farmer has already introduced his quota of foreign weeds; but he also has some formidable natives to combat,—plants that thrive exceedingly with cultivation. *Amphiachyris dracunculoides*, before mentioned; *Croton capitatum*, and, perhaps, worst of all, *Solanum rostratum*, a species that probably combines the bad characters of *Datura Stramonium*, the clot bur and the sand bur. The plant is an annual, and is already easily established, particularly in the neighborhood of Leavenworth City. The common native *Polygonums* in many localities have grown so abundantly and so large in the fields as to make them almost inaccessible to a nervous pedestrian.

Setaria glauca, (the fox tail) was plentifully introduced on almost every farm in some localities, and flourishes exceedingly. Some of the farmers believe it to be native, so unaccountably has it appeared on their farms. But, like purslane, it insidiously marches westward, and often, doubtless, the seeds are abiding their time for the conditions of soil necessary to their development, for years in advance of the agriculturist. But of the introduced plants we sought in vain through all the Middle and Southern counties for *Poa pratensis*, the Kentucky blue grass. Why a plant that is so profitable to agriculture, so tenacious of life, and so encroaching on the neighboring species that few things are able to survive its neighborship (even the hazel with the aid of grazing stock is gradually dying out before it in Illinois, and in most parts, especially of central Illinois, the native grasses have long since yielded to its persistent encroachments),—why this species should be entirely absent was left to us without explanation. The residence grounds in the city of Lawrence, many of them tastefully, elegantly, and expensively improved with buildings, were waving with *Panicum capillare*, varied with species of weeds.

Can it be that all this comes from simple neglect of introduction? In the bushy regions in the neighborhood of Leavenworth and Atchison, some blue grass is met on the roadsides; a few pastures near Leavenworth (looking as if doing well) and the city residence lots are frequently occupied with it; but in no other parts of the State was the plant seen.

Prairie Vegetation.—The following catalogues represent the entire native flora on the areas and at the localities mentioned. Soon most of these things will have passed away before the restless plowshare and the all-devouring kine. I give them as a natural history record.

One hundred square feet on high prairie in Jefferson county:

Sorghum nutans, 20 per cent.
Andropogon scoparius 60 per cent.
Sporopolus heterolepis 5 " "
Helianthus rigidus 10 per cent.
Euphorbia corollata 3 plants.
Asclepias tuberosa 1 plant.
Petalostemon candidum 3 plants.
Coreopsis palmata 1 plant.
Panicum virgatum 6 plants.
Salvia Pitcheri 1 plant.
Solidago Canadensis 1 plant.
Solidago rigida 1 plant.
Solidago linifolia 1 plant.
Aster multiflorus 10 plants.
Panicum dichotomum 1 plant.
Aster azureus 5 plants.
Liatris scariosa 1 plant.
Liatris squarrosa 1 plant.
Eragrostis capillaris 1 plant.
Lespedeza capitata 1 plant.
Eryngium yuccæfolium 1 plant.
Silphium laciniatum 1 plant.
Phlox pilosa 1 plant.

Same area in Franklin county, high prairie.

Sorghum nutans, 60 per cent.
Andropogon scoparius 20 per cent.
Andropogon furcatus 2 per cent.
Lespedeza capitata 8 plants.
Lactuca elongata 1 plant.
Aster laevis 1 plant.
Aster oblongifolius 3 plants.
Salvia Pitcheri 5 plants.
Liatris scariosa 2 plants.
Bouteloua curtipendula 8 plants.
Panicum virgatum 6 plants.
Erigeron canadense 1 plant.
Kuhnia eupatorioides 1 plant.
Petalostemon violaceum 1 plant.
Rosa blanda 3 plants.
Petalostemon candidum 1 plant.
Silphium laciniatum 2 plants.
Panicum dichotomum 1 plant.
Helianthus rigidus 3 plants.
Euphorbia corollata 1 plant.
Solidago rigida 1 plant.

Same area in Anderson county, high prairie, thin soil.

Andropogon scoparius, 40 per cent.
Andropogon furcatus 1 plant.
Sorghum nutans 30 per cent.
Gerardia asperifolia, 50 plants.
Kœleria cristata, 10 per cent.
Helianthus rigidus 25 plants.
Aster multiflorus 1 plant.
Solidago Missouriensis 2 plants.
Amorpha canescens 1 plant.
Panicum panciflorum 3 plants.
Linum Boottii 10 plants.
Solidago linifolia 4 plants.
Polygala verticillata 3 plants.
Baptisia leucantha 1 plant.
Antenaria dioica 3 plants.
Ruellia ciliosa 1 plant.
Coreopsis tinctoria 1 plant.
Psoralea floribunda 1 plant.
Eryngium yuccæfolium 1 plant.

Same area in Linn county; high prairie.

Andropogon furcatus 80 per cent.	Liatris squarrosa 8 plants.
Andropogon scoparius 5 per cent.	Euphorbia corollata 1 plant.
Sorghum nutans 8 per cent.	Panicum dichotomum 3 plants.
Lespedeza capitata 5 plants.	Kœleria cristata 1 plant.
Solidago Missouriensis 6 plants.	Oxalis violacea 1 plant.
Ceanothus ovalis 1 plant.	Echinacea angustifolia 1 plant.
Aster azureus 3 plants.	Linum Boottii 1 plant.
Aster oblongifolius 5 plants.	Polytænia Nuttallii 1 plant.
Solidago linifolia 1 plant.	Bouteloua curtipendula 1 plant.
Salvia Pitcheri 1 plant.	Ambrosia pycnostachya 1 plant.
Amorpha canescens 5 plants.	Petalostemon violaceum 1 plant.
Helianthus rigidus 3 plants.	Polygala incarnata 1 plant.

ART. V.—*Notice of some Minerals from New Jersey;* by Prof. W. T. RŒPPER, of Bethlehem, Pa.

1. *Iron, Manganese, Zinc, Chrysolite.*

THE Stirling Hill, Sussex county, N. J., which with its neighbor, the Minehill, seems to be an inexhaustible storehouse of interesting minerals, both scientifically and commercially, has furnished an antitype to Prof. Brush's Hortonolite from the adjoining Orange county.

Some years ago I examined a black crystalline massive mineral from this locality, and found it to be a unisilicate of the protoxyds of iron, manganese, zinc and magnesium, and as it showed many of the characteristic physical and chemical properties of chrysolitic minerals, especially that peculiar mottled coloring, which is so marked in olivines, I supposed it a variety of tephroite, that peculiar subspecies of the group having shortly before been rediscovered by Prof. Brush. During a visit to the locality in the course of last year, I succeeded in finding distinct crystals, which at once ranged the mineral unmistakably among the chrysolites.

Crystallization.—The crystals occur in great numbers, grouped together, and of all sizes, from an eighth of an inch to two inches in length and nearly one inch in breadth. They are mostly rounded, owing to an incipient alteration of the surface through meteoric waters, black and dull on the outside, but with lustrous and brilliant cleavages on being broken. Some of them, however, are sharp, and allow of a measurement of angles at least by the hand-goniometer. The dominant forms are: $i\bar{2}$ (angle over $i\breve{i}$, 130°), $i\breve{i}$ ($i\bar{2} \wedge i\breve{i}$, 115°), $1\bar{1}$ (angle at top, 77°). Generally subordinate I have observed the following forms: $i\breve{i}$, 1-$\breve{i}$, 1-$\bar{2}$, O, and a face replacing combination edge $i\text{-}\bar{2} \wedge 1\text{-}\bar{1}$ with parallel

intersection-edges, in which therefore $n' = \frac{2m'}{m'-m}$, to which among simple coefficients the forms 2-$\bar{4}$ or 3-$\bar{3}$ would answer. The face is too small and dull to allow of a measurement of angle; 2-$\bar{4}$ appears however the more likely form, as chrysolites seem to have a preference for the ratio 1:2. *Cleavages*, three rectangular; O and i-$\bar{i}$ very and almost equally eminent, with vitreo-pearly luster approaching the sub-adamantine; i-$\bar{i}$ splintery. *Hardness*=5·5–6. *Sp. G.*=3·95–4·08. The average of nine determinations with Jolly's spring balance gave 4·023. *Color*, dark green to black but eminently mottled, so that thin splinters or laminæ transmit a pale yellow light. *Streak*, light-yellowish-reddish-gray. The powder is slightly attracted by the magnet. BB. rather refractory, fusing at thin edges to a dull black slag. On charcoal gives a zinc coating, more distinctly on addition of soda. With the fluxes the usual reactions for silica, iron and manganese. The borax and microcosmic beads give in the O. F. the characteristic brownish purple color, indicating mixtures of iron and manganese, which becomes green in the reducing flame. With acids gelatinizes readily and completely. Some specimens leave a bright green undissolved residue, which I judge to be spinel both from its hardness, its not being attacked by fusion with soda, and complete decomposition by bisulphate of soda.

In the following analyses the silica was separated in the usual manner, the filtrate from the silica neutralized by carbonate of soda, then acidified with acetic acid and a current of sulphuretted hydrogen passed through the solution, which separated the zinc as sulphid. This was filtered off, redissolved in HCl, and then precipitated from the uninterruptedly boiling solution by slowly adding $\dot{N}a\dot{C}$, and the $\dot{Z}n\dot{C}$ finally converted into $\dot{Z}n$ by ignition. The filtrate after the separation of the zinc was then boiled with the addition of $1KO,ClO_5$ to sesquioxydize the iron, the iron precipitated in the usual manner as subacetate, redissolved and reprecipitated by ammonia, the manganese separated by bromine and determined as pyrophosphate with the precautions pointed out by Dr. Gibbs, (this Journal, No. **xxxi**, p. 216), and lastly the magnesia determined as pyrophosphate. I will here remark, that I did not succeed in separating the oxyd of zinc from the iron by the usual acetate of soda process, but that a great and often the greater part of the $\dot{Z}n$ went down with the iron, which I attribute to the necessary boiling of a dilute solution (vid. Johnson's Qual. Fresenius). Hence my former analyses were not correct, gave too little zinc and resulted in uni-oxygen ratio only on account of the near proximity of the equivalents of iron, manganese and zinc-oxyds. I may, however, not have handled the method correctly.

The samples No. 1 and 2 were fresh pieces of cleavage crystals carefully examined by the lens so as to avoid all visible admixtures. No. 1 was lighter in color than No. 2, *a* and *b*, which latter are analyses of the same powder. No. 3 are analyses of two different powders of the massive variety.

	1.	2.		3.		Oxygen				
		a	*b*	*a*	*b*	1.	2, *a*.	2, *b*.	3, *a*.	3, *b*.
$\dot{S}i$	30·76	29·90	30·56	30·67	30·42	16·41	15·95	16·30	16·36	16·22
$\dot{F}e$	33·78	35·60	35·44	35·37	34·20	7·51	7·91	7·88	7·86	7·60
$\dot{M}n$	16·25	16·90	16·93	17·81	17·67	3·66	3·81	3·82	4·01	3·98
$\dot{Z}n$	10·96	10·66	10·70	9·87	9·09	2·16	2·11	2·11	1·95	1·80
$\dot{M}g$	7·60	5·81	5·44	5·69	6·49	3·04	2·32	2·18	2·28	2·60
Insol.		1·03	1·04	1·39	2·65					
	99·35	99·90	100·11	100·80	100·52	16·37	16·15	15·99	16·10	15·98

The foregoing oxygen ratios make the mineral a unisilicate. The crystallization being orthorhombic with the parametric ratios of the chrysolite group, which is confirmed by the other physical and chemical characters; it is hence an iron-manganese-*zinc* chrysolite, the first, to the best of my knowledge, of the group, into the composition of which zinc enters as a constituent.

It occurs, as before said, on Stirling hill, accompanied by Willemite, Franklinite, Jeffersonite and spinel.

2. *Manganesian Dolomite.*

In the vast vein of Willemite, which is being worked on Minehill by the New Jersey Zinc Company, there occur small masses of a beautiful delicate pink mineral with a rhombohedral cleavage, which by their contrast with the purely apple-green Willemite make exceedingly pretty specimens. An analysis gave the following composition:

$\dot{C}a\ddot{C}$	$\dot{M}n\ddot{C}$	$\dot{F}e\ddot{C}$	$\dot{M}g\ddot{C}$	Insol.	
50·40	43·54	·76	5·69	0·08	= 100·47

Specific gravity = 3·052. Hardness = 4.

The mineral differs from the known dialogites by its greater proportion of carbonate of lime, and may be considered either as a dialogite in which a little more than one-half of the $\dot{M}n$ is replaced by lime, or as a dolomite in which about five-sixths of the magnesia is replaced by $\dot{M}n$.

3. *A pseudomorph of opal after a micaceous mineral probably some chlorite.*

On Scotch mountain, Warren county, N. J., not far from New Village, among the Laurentian syenitic gneiss formation of that region, there occur, scattered over the ground, numerous masses of a white quartzose mineral apparently of agglutinated rounded granules of about $\frac{1}{8}$ inch diameter. Upon close examination,

many of these granules show distinct cleavages, which exhibit a hexagonal outline. Searching the ground carefully I found wormlike contorted crystals, in shape like the similar forms of some chloritic minerals. The substance is distinguished from quartz by its low specific gravity =1·961, and inferior hardness (near 6). It is mostly soluble in caustic potash, leaving only 8 per cent insoluble, which seemed to consist, in part at least, of the original mineral. On ignition it loses 7·27 per cent water. It is therefore manifestly amorphous quartz or opal. Indeed small masses of unquestionable opal of various colors are found in the neighborhood.

It hence appears, that micaceous structure is not, as is frequently assumed, the absolute closing scene of the metamorphism of minerals, but that the replacing power of silica is able to overcome the antimetamorphic energies of minerals even, which have arrived at the micaceous stage.

Bethlehem, April 22, 1870.

Art. VI.—*On the Size of Atoms;* by Prof. Sir W. Thomson, F.R.S.*

The idea of an atom has been so constantly associated with incredible assumptions of infinite strength, absolute rigidity, mystical actions at a distance, and indivisibility, that chemists and many other reasonable naturalists of modern times, losing all patience with it, have dismissed it to the realms of metaphysics, and made it smaller than "anything we can conceive." But if atoms are inconceivably small, why are not all chemical actions infinitely swift? Chemistry is powerless to deal with this question, and many others of paramount importance, if barred by the hardness of its fundamental assumptions, from contemplating the atom as a real portion of matter occupying a finite space, and forming a not immeasurably small constituent of any palpable body.

More than thirty years ago naturalists were scared by a wild proposition of Cauchy's, that the familiar prismatic colors proved the "sphere of sensible molecular action" in transparent liquids and solids to be comparable with the wave-length of light. The thirty years which have intervened have only confirmed that proposition. They have produced a large number of capable judges; and it is only incapacity to judge in dynamical questions that can admit a doubt of the substantial correctness of Cauchy's conclusion. But the "sphere of molecular action" conveys no very clear idea to the non-mathe-

* From Nature, No. 22, March 31.

matical mind. The idea which it conveys to the mathematical mind is, in my opinion, irredeemably false. For I have no faith whatever in attractions and repulsions acting at a distance between centers of force according to various laws. What Cauchy's mathematics really proves is this: that in palpably homogeneous bodies, such as glass or water, contiguous portions are not similar when their dimensions are moderately small fractions of the wave-length. Thus in water, contiguous cubes, each of one one-thousandth of a centimeter breadth, are sensibly similar. But contiguous cubes of one ten-millionth of a centimeter must be very sensibly different. So in a solid mass of brickwork, two adjacent lengths of 20,000 centimeters each, may contain, one of them nine hundred and ninety-nine bricks and two half bricks, and the other one thousand bricks: thus two contiguous cubes of 20,000 centimeters breadth may be considered as sensibly similar. But two adjacent lengths of forty centimeters each might contain, one of them one brick and two half bricks, and the other two whole bricks; and contiguous cubes of forty centimeters would be very sensibly dissimilar. In short, optical dynamics leaves no alternative but to admit that the diameter of a molecule, or the distance from the center of a molecule to the center of a contiguous molecule in glass, water, or any other of our transparent liquids and solids, exceeds a ten-thousandth of the wave-length, or a two-hundred-millionth of a centimeter.

By experiments on the contact electricity of metals made eight or ten years ago, and described in a letter to Dr. Joule, which was published in the Proceedings of the Literary and Philosophical Society of Manchester, I found that plates of zinc and copper connected with one another by a fine wire attract one another, as would similar pieces of one metal connected with the two plates of a galvanic element, having about three-quarters of the electro-motive force of a Daniel's element.

Measurements published in the Proceedings of the Royal Society for 1860 showed that the attraction between parallel plates of one metal held at a distance apart small in comparison with their diameters, and kept connected with such a galvanic element, would experience an attraction amounting to two ten-thousand-millionths of a gram weight per area of the opposed surfaces equal to the square of the distance between them. Let a plate of zinc and a plate of copper, each a centimeter square and a hundred-thousandth of a centimeter thick, be placed with a corner of each touching a metal globe of a hundred-thousandth of a centimeter diameter. Let the plates, kept thus in metallic communication with one another be at first wide apart, except at the corners touching the little globe, and let them then be gradually turned round

till they are parallel and at a distance of a hundred-thousandth of a centimeter asunder. In this position they will attract one another with a force equal in all to two grams weight. By abstract dynamics and the theory of energy, it is readily proved that the work done by the changing force of attraction during the motion by which we have supposed this position to be reached, is equal to that of a constant force of two grams weight acting through a space of a hundred-thousandth of a centimeter; that is to say, to two hundred-thousandths of a centimeter-gram. Now let a second plate of zinc be brought by a similar process to the other side of the plate of copper; a second plate of copper to the remote side of this second plate of zinc, and so on till a pile is formed consisting of 50,001 plates of zinc and 50,000 plates of copper, separated by 100,000 spaces, each plate and each space one hundred-thousandth of a centimeter thick. The whole work done by electric attraction in the formation of this pile is two centimeter-grams.

The whole mass of metal is eight grams. Hence the amount of work is a quarter of a centimeter-gram per gram of metal. Now 4,030 centimeter-grams of work, accord-ing to Joule's dynamical equivalent of heat, is the amount required to warm a gram of zinc or copper by one degree centigrade. Hence the work done by the electric attraction could warm the substance by only $\frac{1}{1000}$ of a degree. But now let the thickness of each piece of metal and of each intervening space be a hundred-millionth of a centimeter instead of a hundred-thousandth. The work would be increased a million-fold unless a hundred-millionth of a centimeter approaches the smallness of a molecule. The heat equivalent would therefore be enough to raise the temperature of material by 62°. This is barely, if at all, admissible, according to our present knowledge, or, rather, want of knowledge, regarding the heat of combination of zinc and copper. But suppose the metal plates and intervening spaces to be made yet four times thinner, that is to say, the thickness of each to be four-hundred-millionth of a centimeter. The work and its heat equivalent will be increased sixteen-fold. It would therefore be 990 times as much as that required to warm the mass by 10 cent, which is very much more than can possibly be produced by zinc and copper in entering into molecular combination. Were there in reality anything like so much heat of combination as this, a mixture of zinc and copper powders would, if melted in any one spot, run together, generating more than heat enough to melt each throughout; just as a large quantity of gunpowder if ignited in any one spot burns throughout without fresh application of heat. Hence plates of zinc and copper of a three-hundred-millionth of a centimeter thick, placed close together alternately, form a near approximation to

a chemical combination, if indeed such thin plates could be made without splitting atoms.

The theory of capillary attraction shows that when a bubble —a soap-bubble for instance—is blown larger and larger, work is done by the stretching of a film which resists extension as if it were an elastic membrane with a constant contractile force. This contractile force is to be reckoned as a certain number of units of force per unit of breadth. Observations of the ascent of water in capillary tubes shows that the contractile force of a thin film of water is about sixteen milligrams weight per millimeter of breadth. Hence the work done in stretching a water film to any degree of thinness, reckoned in millimeter-milligrams, is equal to sixteen times the number of square millimeters by which the area is augmented, provided the film is not made so thin that there is any sensible diminution of its contractile force. In an article "On the Thermal effect of drawing out a Film of Liquid," published in the Proceedings of the Royal Society for April, 1858, I have proved from the second law of thermodynamics that about half as much more energy, in the shape of heat, must be given to the film to prevent it from sinking in temperature while it is being drawn out. Hence the intrinsic energy of a mass of water in the shape of a film kept at constant temperature increases by twenty-four milligram-millimeters for every square millimeter added to its area.

Suppose then a film to be given with a thickness of a millimeter, and suppose its area to be augmented ten thousand and one fold: the work done per square millimeter of the original film, that is to say per milligram of the mass, would be 240,000 millimeter-milligrams. The heat equivalent of this is more than half a degree centigrade of elevation of temperature of the substance. The thickness to which the film is reduced on this supposition is very approximately a ten-thousandth of a millimeter. The commonest observation on the soap-bubble (which in contractile force differs no doubt very little from pure water) shows that there is no sensible diminution of contractile force by reduction of the thickness to the ten-thousandth of a millimeter; inasmuch as the thickness which gives the first maximum brightness round the black spot seen where the bubble is thinnest, is only about an eight-thousandth of a millimeter.

The very moderate amount of work shown in the preceding estimates is quite consistent with this deduction. But suppose now the film to be further stretched, until its thickness is reduced to a twenty-millionth of a millimeter. The work spent in doing this is two thousand times more than that which we have just calculated. The heat equivalent is 1,130 times

the quantity required to raise the temperature of the liquid by one degree centigrade. This is far more than we can admit as a possible amount of work done in the extension of a liquid film. A smaller amount of work spent on the liquid would convert it into vapor at ordinary atmospheric pressure. The conclusion is unavoidable, that a water-film falls off greatly in its contractile force before it is reduced to a thickness of a twenty-millionth of a millimeter. It is scarcely possible, upon any conceivable molecular theory, that there can be any considerable falling off in the contractile force as long as there are several molecules in the thickness. It is therefore probable that there are not several molecules in a thickness of a twenty-millionth of a millimeter of water.

The kinetic theory of gases suggested a hunded years ago by Daniel Bernouilli has, during the last quarter of a century, been worked out by Herapath, Joule, Clausius, and Maxwell, to so great perfection that we now find in it satisfactory explanations of all non-chemical properties of gases. However difficult it may be even to imagine what kind of thing the molecule is, we may regard it as an established truth of science that a gas consists of moving molecules disturbed from rectilineal paths and constant velocities by collisions or mutual influences, so rare that the mean length of proximately rectilineal portions of the path of each molecule is many times greater than the average distance from the center of each molecule to the center of the molecule nearest it at any time. If, for a moment, we suppose the molecules to be hard elastic globes all of one size, influencing one another only through actual contact, we have for each molecule simply a zigzag path composed of rectilineal portions, with abrupt changes of direction. On this supposition Clausius proves, by a simple application of the calculus of probabilities, that the average length of the free path of a particle from collision to collision bears to the diameter of each globe, the ratio of the whole space in which the globes move, to eight times the sum of the volumes of the globes. It follows that the number of the globes in unit volume is equal to the square of this ratio divided by the volume of a sphere whose radius is equal to that average length of free path. But we cannot believe that the individual molecules of gases in general, or even of any one gas, are hard elastic globes. Any two of the moving particles or molecules must act upon one another somehow, so that when they pass very near one another they shall produce considerable deflexion of the path and change in the velocity of each. This mutual action (called force) is different at different distances, and must vary, according to variations of the distance, so as to fulfil some definite law. If the particles were hard elastic globes acting

upon one another only by contact, the law of force would be —zero force when the distance from center to center exceeds the sum of the radii, and infinite repulsion for any distance less than the sum of the radii. This hypothesis, with its "hard and fast" demarcation between no force and infinite force, seems to require mitigation. Without entering on the theory of vortex atoms at present, I may at least say that soft elastic solids, not necessarily globular, are more promising than infinitely hard elastic globes. And, happily, we are not left merely to our fancy as to what we are to accept for probable in respect to the law of force. If the particles were hard elastic globes, the average time from collision to collision would be inversely as the average velocity of the particles. But Maxwell's experiments on the variation of the viscosities of gases with change of temperature prove that the mean time from collision to collision is independent of the velocity, if we give the name collision to those mutual actions only which produce something more than a certain specified degree of deflection of the line of motion. This law could be fulfilled by soft elastic particles (globular or not globular); but, as we have seen, not by hard elastic globes. Such details, however, are beyond the scope of our present argument. What we want now is rough approximations to absolute values, whether of time or space or mass—not delicate differential results. By Joule, Maxwell, and Clausius we know that the average velocity of the molecules of oxygen or nitrogen or common air, at ordinary atmospheric temperature and pressure, is about 50,000 centimeters per second, and the average time from collision to collision a five-thousand-millionth of a second. Hence the average length of path of each molecule between collisions is about $\frac{1}{10000}$ of a centimeter. Now, having left the idea of hard globes, according to which the dimensions of a molecule and the distinction between collision and no collision are perfectly sharp, something of apparent circumlocution must take the place of these simple terms.

First, it is to be remarked that two molecules in collision will exercise a mutual repulsion in virtue of which the distance between their centers, after being diminished to a minimum, will begin to increase as the molecules leave one another. This minimum distance would be equal to the sum of the radii, if the molecules were infinitely hard elastic spheres; but in reality we must suppose it to be very different in different collisions. Considering only the case of equal molecules, we might, then, define the radius of a molecule as half the average shortest distance reached in a vast number of collisions. The definition I adopt for the present is not precisely this, but is chosen so as to make as simple as possible the statement I have to

make of a combination of the results of Clausius and Maxwell. Having defined the radius of a gaseous molecule, I call the double of the radius the diameter; and the volume of a globe of the same radius or diameter I call the volume of the molecule.

The experiments of Cagniard de la Tour, Faraday, Regnault, and Andrews, on the condensation of gases do not allow us to believe that any of the ordinary gases could be made forty thousand times denser than at ordinary atmospheric pressure and temperature, without reducing the whole volume to something less than the sum of the volume of the gaseous molecules, as now defined. Hence, according to the grand theorem of Clausius quoted above, the average length of path from collision to collision cannot be more than five thousand times the diameter of the gaseous molecule; and the number of molecules in unit of volume cannot exceed 25,000,000 divided by the volume of a globe whose radius is that average length of path. Taking now the preceding estimate, $\frac{1}{1\,0\,0\,0\,0\,0\,0\,0}$ of a centimeter, for the average length of path from collison to collision, we conclude that the diameter of the gaseous molecule cannot be less than $\frac{1}{2\,0\,0\,0\,0\,0\,0\,0\,0\,0\,0}$ of a centimeter; nor the number of molecules in a cubic centimeter of the gas (at ordinary density) greater than $6 \times 10^{2\,1}$ (or six thousand million million million).

The densities of known liquids and solids are from five hundred to sixteen thousand times that of atmospheric air at ordinary pressure and temperature; and, therefore, the number of molecules in a cubic centimeter may be from $3 \times 10^{2\,4}$ to $10^{2\,6}$ (that is, from three million million million million to a hundred million million million million). From this (if we assume for a moment a cubic arrangement of molecules), the distance from center to nearest center in solids and liquids may be estimated at from $\frac{1}{1\,0\,0\,0\,0\,0\,0\,0}$ to $\frac{1}{2\,0\,0\,0\,0\,0\,0\,0\,0}$ of a centimeter.

The four lines of argument which I have now indicated, lead all to substantially the same estimate of the dimensions of molecular structure. Jointly they establish with what we cannot but regard as a very high degree of probability the conclusion that, in any ordinary liquid, transparent solid, or seemingly opaque solid, the mean distance between the centers of contiguous molecules is less than the hundred-millionth, and greater than the two thousand-millionth of a centimeter.

To form some conception of the degree of coarse-grainedness indicated by this conclusion, imagine a rain drop, or a globe of glass as large as a pea, to be magnified up to the size of the earth, each constituent molecule being magnified in the same proportion. The magnified structure would be coarser grained than a heap of small shot, but probably less coarse grained than a heap of cricket-balls.

ART. VII.—*Miscellaneous Optical Notices*; by WOLCOTT GIBBS, M.D., Rumford Professor in Harvard University.

§ 1.

*On the measurement of wave lengths by means of indices of refraction.**

IN a brief notice† communicated to the British Association for the Advancement of Science at its meeting in 1849, Prof. Stokes has given a method for measuring wave lengths, which depends upon the fact that, in substances of medium refractive power, the increment of the index of refraction in passing from one point of the spectrum to another is nearly proportional to the increment of the square of the reciprocal of the wave length. The author showed that even when the intervals were taken much longer than necessary, the error in the wave length was usually only in the eighth place of decimals. At the date of the publication of this notice the subject of wave lengths possessed but little interest. The recent development of the spectral analysis of light has given a new impulse to this branch of optics, and has rendered necessary the construction of a normal map of the entire solar spectrum. This has been most successfully accomplished by Angström,‡ but an attentive study of his work, as well as of the elaborate researches of Van der Willigen§ and Ditscheiner,‖ will show that new measurements will be far from superfluous. The imperfections even of the best ruled glasses are so great that it may be reasonably doubted whether the wave lengths of very fine lines can be satisfactorily measured directly. Methods of determining such wave lengths, depending upon the comparison of the refraction and diffraction spectra, have been given by myself¶ and by Thalen.** As it seems at least desirable to multiply such methods, I will here give first a discussion of the method of Stokes in its original form, and afterward a simplification of that method which will also have its uses.

If Cauchy's formula for dispersion, $n=a+\frac{b}{\lambda^2}+\frac{c}{\lambda^4}$,

* Read before the National Academy of Sciences, April 12th, 1870.

† Report of the British Association for the Advancement of Science for 1849. Notices and abstracts, p. 10.

‡ Recherches sur le Spectre Solaire. Berlin, 1869.

§ Archives du Musée Teyler, vol. i, p. 1.

‖ Sitzungsberichte der k. k. Akad. der Wissenschaften Bd. l, 1864.

¶ This Journal, xlvii, March, 1869.

** Mémoire sur la determination des longueurs d' onde des raies métalliques, 1868.

be reduced to its two first terms, and if we then eliminate the constants a and b from three equations of the form

$$n = a + \frac{b}{\lambda^2}$$

we shall obtain the three following equations, involving only wave lengths and indices of refraction:

$$\lambda_1^2 = \frac{(n_3 - n_2)}{(n_3 - n_1)\frac{1}{\lambda_2^2} + (n_2 - n_1)\frac{1}{\lambda_3^2}} \qquad (1)$$

$$\lambda_2^2 = \frac{(n_3 - n_1)}{(n_2 - n_1)\frac{1}{\lambda_3^2} + (n_3 - n_2)\frac{1}{\lambda_1^2}} \qquad (2)$$

$$\lambda_3^2 = \frac{(n_2 - n_1)}{(n_3 - n_1)\frac{1}{\lambda_2^2} + (n_3 - n_2)\frac{1}{\lambda_1^2}} \qquad (3)$$

Of these equations (1) and (3) serve for extrapolation and (2) for interpolation. To test the degree of accuracy attainable in determining wave lengths by these formulas, I have selected the measurements made by Van der Willigen.* The indices of refraction determined by the Dutch physicist are in fact the only indices which are at once sufficiently exact and sufficiently numerous. In addition they have the great advantage of having been made with reference to lines in the solar spectrum the wave lengths of which had been measured by the same observer. There can therefore be no question of identity. As a first example of the method, I give a determination of the wave length of C, taking B as one of the lines exterior to C, and taking in succession 7 other exterior lines more refrangible than C, to combine with B. Formula (2) was therefore employed, and with the following data and results:—

B	1·61079	687·48		
C	1·61252	656·56		
D	1·61436	628·11	656·70	+0·14
11	1·61537	613·96	656·71	+0·15
13	1·61560	610·52	656·56	0·00
14	1·61728	589·56	656·71	+0·15
16	1·61978	561·80	656·76	+0·20
17	1·62064	553·19	656·79	+0·23
19	1·62143	545·83	656·87	+0·31
			Mean of the errors	+0·17

In this table the first column gives the designation or number of the lines, the second its index of refraction, as determined by a Steinheil prism of 60°, the third the corresponding

* Archives du Musée Teyler, vol. i, p. 70.

wave length, according to Van der Willigen, and the fourth the wave length as found by formula (2) by combining B with each line after C in succession.

The mean of the seven values of the wave length of C thus found is 656·70, which is in excess of Van der Willigen's own determination of the value of C by 0·14. From this it appears that the method may be applied with a tolerable degree of approximation, even in the case of a flint glass prism of high dispersive power, and for indices of refraction which refer to lines at considerable angular distances from each other. The increase in the computed values of C, as the intervals between B and the second line of comparison are increased, will however clearly appear from the table. The following results were obtained with the indices of a second Steinheil prism, No. 2, of 46° 52′ 25″·8, also of flint glass.

B	1·60521	B and 8*a*	656·21	−0·35
C	1·60694	B and 11	656·33	−0·23
8*a*	1·60872	B and 13	656·31	−0·25
11	1·60973	B and 16	656·38	−0·18
13	1·60998	B and 17	656·47	−0.09
16	1·61408			
17	1·61495			

The mean of which is 656·28, the error being −0·28. To determine to what extent the method applies, when flint glass prisms are used, and the indices are selected from the more refrangible portion of the spectrum, the following data were assumed:—

F	1·62917	486·39	F and G	438·88	+0.30
35	1·63244	467·00	F and 39	438·76	+0·18
37	1·63828	438·58	F and 38	438·82	+0·24
38	1·63931	434·28	G and 35	438·76	+0·18
39	1·63965	432·74	35 and 38	438·75	+0·17
G	1·64006	431·12	35 and 39	438·67	+0.09

In this table, line 37 is taken as the middle line in applying formula (2), and the results obtained by combining the other lines in pairs are given in columns 4, 5 and 6. It will be seen that, as in the case of the less refrangible portion of the spectrum, the results obtained are with this prism always too high. For the purpose of comparison, I have computed the same wave length from the indices of refraction of the second prism. The data and results are as follows:—

F	1·62332	486·39	F and G	439·07	+0·49
35	1·62657	467·00	F and 39	438·89	+0·31
37	1·63221	438·58	F and 38	438·92	+0·34
38	1·63324	434·28	G and 35	439·00	+0·42
39	1·63358	432·74	35 and 38	438·89	+0·31
G	1·63400	431·12	35 and 39	438·84	+0·26

In the case of the first prism the mean of the errors is +0·21, while for the second the mean of the errors is +0·35. From this it appears that in the more refrangible portion of the spectrum the errors are considerably greater than in the less refrangible portion, even for equal differences of wave length, and further, that the advantage in precision is with the prism having the higher dispersive power. As the probable errors of the measurements of the indices of refraction are not given, it is impossible to determine to what extent the errors in the computed wave lengths are due solely to want of precision in the indices. It is also to be remarked that, while with the second prism the errors in the less refrangible portion of the spectrum are affected with the sign −, in the more refrangible portion they are largely positive. The close agreement in the value of the wave length of 37, as found by Van der Willigen, with the values as found by Ditscheiner and Angström—438·27 and 438·28—proves that the source of error is not an erroneous determination of this quantity. It seems therefore certain that the nearly constant errors noted above are due in part to the fact that the indices of refraction are determined only to five places of decimals, and in part to the high dispersive powers of the prisms employed, which would render it necessary to employ more than two terms in Cauchy's formula to obtain a closer approximation. As the formulas for interpolation would in this way be rendered extremely complicated, it is better, in the case of any series of observations embracing a particular part of the scale, simply to determine the mean of the errors, and to apply this mean with its proper sign to the computed values of the particular wave length to be determined by the measurement of indices of refraction. If we apply such a correction in the cases of the four series of data and results given above, we find for the corrected values of the wave-lengths the following numerical results:—

(1)	(2)	(3)	(4)
656·53	656·49	438·67	438·72
656·54	656·61	438·55	438·54
656·38	656·59	438·61	438·57
656·54	656·66	438·55	438·65
656·59	656·75	438·54	438·54
656·62		438·44	438·49

The true values being respectively 656·56 and 438·56. These results are, I think, sufficient to show that a valuable control for the accuracy of measurements of wave lengths may be obtained even when prisms of high dispersive power are employed, provided that the intervals taken are not too large. It seems at least probable that a greater degree of precision is attainable in measuring indices in the case of substances of high

than in those of low dispersive power, partly because the angular deviations to be measured are larger, and partly because the spectral lines are less crowded together.

The following example will serve to illustrate the advantage of taking shorter intervals:—

Lines.	λ	Indices.	λ	Indices.	λ
25*b*	518·63	1·62459	-----	1·61882	-----
26	517·51	1·62472	517.61	1·61895	517·56
27*β*	517·07	1·62479	-----	1·61901	-----

The data are here also taken from Van der Willigen's measures with the same prisms.

When the angular distances between three spectral lines are not too great, the angular deviation of the lines may, as I find, be substituted for the indices of refraction in formulas (1), (2) and (3). The differences between the angular deviations are of course to be converted into seconds. The following results will show the degree of accuracy attainable by this method, the data being taken from Ditscheiner's* measurements of the indices of a flint prism by Steinheil of refracting angle 60° 4′ 59″.

Kirchhoff's scale.	λ	Angular deviations.	λ	Indices.	λ
B 593	687·06	47° 40′ 55″	-----	1·61358	-----
C 694	655·95	47° 51′ 19″	655·97	1·61537	655.82
-- 877	613·57	48° 8′ 8″	-----	1·61824	-----

From this it appears that the error in the determination of the wave-length of the middle line C is only +0·02 when the angular deviations are employed, but amounts to −0·13 when the indices of refraction are taken as the elements of the calculation. Yet the interval between B and 877 is very large.

The following data are taken from another part of the scale, the measurements being made with the same prism:—

Kirchhoff's scale.	λ	Angular deviations.	λ	Indices.	λ
b 1648·8	517·13	49° 4′ 16″	-----	1·62775	-----
1655·6	516·58	49° 4′ 44″	516·56	1·62782	516·61
1693·8	514·08	49° 6′ 47″	-----	1·62817	-----

Hence the error in the determination of the wave length of 1655·6 is, when the angular deviations are taken, only −0·02 and when the indices are taken +0·03. It must be borne in mind that in all the above mentioned examples the angles are those of minimum deviation. As the numbers upon Kirchhoff's scale also represent angular though not *minimum* deviations, it seemed worth while to determine how far for a short interval, these could be employed. Taking the three scale numbers of the last example, the error in the wave-length of

* Bestimmung der Wellenlängen der Fraunhoferschen Linien des Sonnenspectrums, p. 43.

1655·6 was found to be −0·38, and when the scale numbers were taken as the sines or tangents of corresponding angles, +0·09.

The following data are taken from the more refrangible part of the spectrum, the measurements being also those of Ditscheiner, and made with the same prism:—

	2822·8	433.34	50° 34′ 57″	-----	1·64287	-----
G	2854·7	430·88	50° 37′ 52″	430·68	1·64334	430·83
	2969·7	429·90	50° 38′ 47″	-----	1·64352	-----

In this case the error in the wave-length of the middle line, 2854·7, is −0·20, as determined from the angular deviations, and −0·05, as determined from the indices. It must be borne in mind that, in this part of the spectrum, the determination both of wave lengths and of indices of refraction is difficult on account of the feeble intensity of the light.

Since only the differences between the angular deviations of the spectral lines are employed in the formulas above given, it follows that in determining wave lengths by the method in question, it is not necessary to employ a spectrometer with a divided circle and appliances for the measurement of large angles. A common spectroscope will be sufficient, if the observing telescope be provided with a filar micrometer by means of which the angular distances of any given line from two other lines of which the wave lengths are known may be measured. The researches of Angström leave nothing to be desired as regards the wave-lengths of standard lines, and the method given may prove a convenient means of determining with all requisite precision the wave lengths of metallic lines.

§ 2.

On liquids of high dispersive power.—Of the liquids which have hitherto been proposed for the construction of prisms, bisulphid of carbon unquestionably presents the greatest advantages. It is cheap, colorless, and unites a moderately high mean refractive to a very high dispersive power. By tacit consent a prism of 60° filled with this liquid has come to be adopted as a sort of standard. The disadvantages of the bisulphid are equally well known, and I have spent no little time and labor in the endeavor to find a liquid with a still higher dispersive power, less volatile, less sensitive optically to changes of temperature, and less offensive in odor. In these efforts I have not been altogether successful, no one liquid examined possessing all the qualities desired. Many organic liquids with high dispersive powers are difficult to prepare in a state of purity and speedily become discolored by absorption of oxygen from the air. Such are oil of cassia, the colorless oil obtainable from balsam of

Peru and others. The thallic alcohol of Lamy* is far too costly. The solution of silico-tungstate of sodium,† of meta-tungstate of sodium ‡ and of soluble tungstic acid § as obtained by dialysis, all promised good results from their extraordinary densities, but all proved difficult to prepare in a state of purity and extremely easy of decomposition.

A solution of phosphorus in bisulphid of carbon has, according to Messrs. Dale and Gladstone,‖ a dispersion of 0·225,¶ or nearly one and a half times as great as bisulphid of carbon alone, but becomes turbid on exposure to sunlight from the formation of amorphous phosphorus. It occurred to me, that, by dissolving sulphur with the phosphorus, the formation of amorphous phosphorus might be prevented, and experiment proved that this was the case. The solution, as thus obtained, has a pale yellow color, but is perfectly clear and undergoes no change by the action of light even when long continued. I have been in the habit of preparing it by dissolving one part of dry flowers of sulphur and two parts of phosphorus, in four or five parts of bisulphid of carbon, and filtering the liquid through a well dried ribbed paper filter, which is easily done. The refractive and dispersive power of the solution will of course vary with the quantity of phosphorus and sulphur dissolved. By a gentle heat the whole, or nearly the whole, of the bisulphid of carbon may be driven off, a liquid compound of sulphur and phosphorus remaining which has so high a mean refractive power that it cannot be employed with prisms having a refractive angle of more than 45°–50°. The same end may, however, also be attained by continually adding phosphorus to a saturated solution of sulphur in bisulphid of carbon, in which phosphorus appears to be soluble without limit.

With a strong and probably saturated solution of sulphur in CS_2 the angle between Li and D was 0° 50′ 10″. When phosphorus was added the angle was 2° 25′ 30″, the refracting angle of the prism being 60°. In this last case the angle between Na_1 and Na_2 was 0° 2′ 20″. The spectrum was perfectly clear, the definition of the dark lines leaving nothing to be desired. In consequence, however, of the yellow color of the liquid there is always a marked absorption of the violet end of the spectrum.

In working with the above described solution I have employed hollow glass prisms with refracting plates cemented on with a mixture of glue and molasses. These were found to

* Ann. de Chimie et de Physique, 4th series, vol. iii, p. 373.
† Ann. de Chimie et de Physique, 4th series, vol. iii, p. 5.
‡ Scheibler in Journal für prakt. Chimie, lxxxiii, p 273.
§ Graham in Journal of the Chemical Society, vol. ii, p. 318
‖ L. and E. Phil. Mag., vol. xviii, p. 30.
¶ The number 0·225 is the difference between the indices for the extreme red and violet rays.

be perfectly tight and to last for months without change. The great disadvantage in the use of a solution of sulphur and phosphorus consists in the danger of breaking the prisms; the liquid taking fire spontaneously when it has been a few seconds in contact with any porous material like wood or paper. On the other hand, however, the large quantity of sulphur present prevents the fire from spreading, a drop placed upon a piece of wood leaving after combustion only a charred spot. When not in use the prisms should be kept in an iron pot with a tight cover. In this manner I have employed and preserved two during a long and hot summer. The viscid, or rather oily, nature of the solution serves to prevent, to a great extent, the formation of ascending and descending currents from slight changes of temperature, and when the prisms are well shaken before use the definition remains perfect for a long time. In my spectroscope the prisms rest upon a plate of glass instead of upon one of metal.

§ 3.

On an advantageous form of apparatus for the study of the absorption of light in colored liquids.

In his examination of the spectra of colored fluids, Mr. Gladstone employed a hollow wedge of glass, the two refracting surfaces of which made with each other an acute angle. The wedge was filled with the liquid to be studied and so placed that the refracting edge of the analyzing prism was at right angles to the line of intersection of the two faces of the wedge. In this manner a beam of light was obtained which represented different thicknesses of the absorbing liquid, and the resulting spectrum became a complete absorption diagram. In using this apparatus I found the angular deviation produced by the wedge a source of considerable inconvenience. In addition it is easy to see that the wedge itself produces a certain amount of chromatic dispersion. To remedy these defects and at the same time retain the advantages of the method, I have devised what may be termed a double wedge. Two hollow wedges, of glass or metal, are placed together in such a manner that the first and last surfaces of the bounding plates of glass are parallel. The two wedges are separated by a single plate of glass with parallel surfaces. The base of each wedge, or acute-angled prism, is bored for the insertion of a cork or stopper. The construction of the apparatus will be readily understood from the diagram. In using it with a colored aqueous solution one of the hollow prisms, or wedges, is to be filled with distilled water, the other with the aqueous solution to be examined. The incident beam of sunlight is then allowed to fall perpendicularly upon

either surface, and the slit of the spectroscope is placed so as to be perpendicular to the two lines of intersection of the three refracting plates of the double wedge. In this manner a complete absorption diagram is obtained by the various thicknesses of the liquid examined. For substances soluble in alcohol, ether, &c., one of the hollow wedges must be filled with the colorless solvent, whatever be its nature. By this means all angular deviation and prismatic dispersion are avoided, as the coloring matter does not sensibly change the refractive power of the liquid in which it is dissolved, and the incident beam passes through without change in direction. In my apparatus the wedges have acute angles of about 15°. This I find to be quite sufficient for most purposes, as it is easy to increase or diminish the quantity of substance dissolved. When it is wished to examine the absorption produced by a definite thickness of liquid or by different thicknesses in succession, the double wedge is to be so placed that the slit of the spectroscope shall be parallel to the lines of intersection of the faces of the wedges. By moving the wedge to one side or the other all thicknesses of liquid, from 0 to the maximum, obtainable with the apparatus used, may be successively examined.

§ 4.

On tests for the perfection and parallelism of plane surfaces of glass.

When a plano-convex lens of long radius of curvature is placed upon a plane surface of glass and the system is illuminated by an obliquely incident beam of monochromatic light as, for example, by a sodium flame, the well known phenomenon of Newton's rings is observed with remarkable distinctness and perfection of definition. The symmetry of the rings will depend, in part, on the perfection of figure of the lens, in part on that of the plane surface. An extremely minute deviation from a perfect plane will produce a marked distortion of the circular figure of the ring nearest the center. That this distortion is, or is not due to the lens may be determined by rotating the lens round its optical axis normal to the plane. No change of figure will be seen if the lens is perfect in form and the inequality is in the plane surface only. Different parts of the plane surface may of course be tested in succession, by moving the lens from point to point, and if necessary the rings may be observed with a telescope.

Prof. Rood, of New York, has suggested for the observation of Newton's rings a method which permits of the employment of a lens of comparatively small radius of curvature and a microscope. In his arrangement the lens and plate of glass are placed upon the stage of the microscope, the light from beneath being cut off, and monochromatic light is then thrown down

upon the system by means of a plate of glass with parallel surfaces inclined to the axis of the microscope at a convenient angle and placed between the objective and the plano-convex lens. In this manner the rings are seen with great distinctness and beauty, and the arrangement is particularly compact and convenient.

The interference bands of Talbot afford a method not merely of observing with great precision the inequalities of surface and want of parallelism of the faces of plates of glass, but also of photographing these defects and obtaining a permanent chart of the glass which may be of material assistance in correcting its figure. It is only necessary for this purpose to place the glass to be examined near to the object glass of the collimator and perpendicular to its axis, so as to intercept that half of the bundle of parallel rays which falls upon the first surface of the first prism nearest its refracting edge. If the plate has perfectly plane and parallel surfaces the interference bands will be sharply defined and parallel in the whole field of view. The slightest inequality of surface, or inclination of the faces, will produce curvature or distortion of the bands, and, if the eyepiece of the observing telescope be removed, the image may be received on a sensitive plate and photographed. The number of prisms to be employed in a particular case will depend upon the thickness of the plate of glass examined and, in general terms, upon its dispersive power. For a piece of French plate glass four millimeters in thickness, two bisulphid of carbon prisms of 60° must be used to produce a sufficient separation of the interference bands to enable them to be seen distinctly. More prisms must be used for thicker plates and in this way a limit is soon reached at which the method ceases to be applicable.

Cambridge, May 1st, 1870.

Art. VIII.—*On the Occurrence of a Peat bed beneath Deposits of Drift in Southwestern Ohio.*

A BED of peat has lately been found one mile east of Germantown, Montgomery county, Ohio, and twelve miles west of south from Dayton—in the occurrence and connections of which there are several facts of unusual interest.

It lies in, and directly above, the channel of Twin creek, a tributary of the Miami river. The general course of the creek is southeasterly, but just above the point where the peat bed is exposed, it has made a sudden change in direction from east to west of south. Its northern and eastern banks for $\frac{1}{4}$ of a mile in each direction from the point of deflection, are precipitous walls of stratified clay and gravel, from 50 to 100 feet in thick-

ness; kept nearly vertical by the constant undermining action of the stream.

Beneath these heavy deposits and occupying 40 rods of the east bank of the creek, the peat bed is found, varying in thickness, in different portions of its extent, from 12 to 20 feet. The amount of the bed that is exposed depends upon the stage of water in the stream. The stream is bedded for 10 or 15 rods upon the peat, but in deeper portions of the channel, upon the eastern bank, an underlying formation of gravel can be detected. The uppermost layers of the peat contain undecomposed sphagnous mosses, grasses and sedges, but in other portions of the bed, the vegetable structure is generally indistinct, with the exception of abundant fragments of coniferous wood, which in many instances can be identified as Red Cedar (*Juniperus virginianus*). At the southern extremity of the bed in particular, there is a great accumulation of wood, in trunks, roots, branches and twigs, much of which has been flattened by the pressure of the 80 feet of clay and gravel that overlie it. Branches that were originally two inches in diameter now afford lenticular sections with no more than a $\frac{1}{4}$ inch for the shorter axis, while many of the smaller stems have been compressed into ribbons. The berries of the cedar are abundant in the upper layers of the peat. At a point $\frac{1}{2}$ mile higher up the stream, trunks of cedar nearly two feet in diameter, have been taken from beneath these same drift beds and turned to account for fencing posts.

There are indications that the peat bed has a considerable extent to the northward and eastward. A bed of "black earth" was found underlying clay and gravel in digging a well $1\frac{3}{4}$ miles east of this locality. The bed occurred at a depth of 30 feet and was itself from 10 to 15 feet in thickness. The waters of springs in the same neighborhood are discolored, as if by contact with such deposits.

It may be added in this connection that there is a large amount of wood buried beneath the drift throughout this region generally. It is not a circumstance of infrequent occurrence to meet with it in the digging of wells. There is scarcely a square mile in the thickly settled portions of the adjacent country in which instances of this kind can not be found, and three instances are on record within the limits of a single village.

The wood is in great part coniferous, but not exclusively so; for according to the testimony of intelligent and observing, practical men who deem themselves entirely competent to give a judgment in the case, ash, hickory and sycamore, together with grape-vines and beech leaves, have been found covered with drift deposits.

A stratum of soil, one or two feet in thickness is often associated with these vegetable remains. The soil and the wood

occur at various depths, but in the cases already noted, between the limits of 10 feet and 90 feet. A large proportion, however, of the instances on record, have been found at about 30 feet in depth, immediately beneath the yellow clays that constitute the last of the drift series in this region.

Through all portions of the peat above mentioned, sand and pebbles are scattered. The pebbles are mostly of small size, seldom larger than a pea, but occasionally three or four inches in diameter. They agree in general character with the gravel of the country.

At the lower extremity of the peat bed, the formation thins out and the bottom layers are found above the water, resting upon a surface of gravel that slopes downward at an angle of about 30 degrees. All the limestone pebbles which the peat overlies at this point, appear to have been "burned." They are white and soft, as much so as they would have been if they had been converted into hydrates of lime by the ordinary processes. Analysis, however, shows them to be in the state of carbonates.

In the inclined strata, heavy beds of ochreous gravel occur. The ochre is easily separated from the gravel by washing and furnishes a marketable paint of fair quality. The nature and arrangement of the materials of these inclined beds indicate that they were brought from the eastward by a torrent-like stream and deposited over a precipitous bank.

In pockets of the gravel and also in the clay that immediately covers the peat, small quantities of vivianite, "blue earth," or phosphate of iron, are found. From one of the largest accumulations of this substance, a tusk or tooth was taken. It was described as resembling a hog's tusk, except that it was much larger. It may also be added that two mastodon tusks, each measuring eight feet in length, were taken in the spring of 1870, from the northern part of the same drift bed to which the peat belongs and at about the same level.

The reference of the phosphoric acid of the vivianite to vertebrate bones will, therefore, hardly be questioned.

From the above named facts, we seem warranted in concluding that the coniferous wood in question grew in the region where we find it buried. The amount of the wood renders this probable and the nature of the remains forbids any other supposition. In this connection, it is only needful to recall the facts, that cedar berries in notable quantity, and that branching twigs, the veriest spray of the cedar, sometimes still covered with bark, are well preserved in the peat.

We learn furthermore that the date, at which this vegetation grew, was in the closing or Champlain epoch of the Drift period, for it is underlain by stratified drift deposits. A subsidence of the continent below its present level had already occurred, dur-

ing which these underlying beds were formed, but there would seem to have been a restoration of this southern border of the drift-swept region at least, to dry land once more, and this restoration must have continued through a period of considerable length. It was followed by another movement of depression, during which the highest of the yellow clays, the latest formation of the drift, were deposited. There seem materials in this line of facts for a more orderly division of the later formed deposits of the post-tertiary than has heretofore been recognized.

We also learn that mammalian life was associated with this intercalated period of vegetable growth. The mammoth and the mastodon subsisted on the coniferous wood which is represented so largely here. The series of changes in level already referred to, must have exterminated these earlier representatives of elephantine life, but we find the same species returning to their old dwelling places when the waters of the drift seas had finally abated.

ART. IX. — *On the Theoretical Temperature of the Sun; under the Hypothesis of a Gaseous Mass maintaining its Volume by its Internal Heat, and depending on the Laws of Gases as known to Terrestrial Experiment;* by J. HOMER LANE, Washington, D. C.

[Read before the National Academy of Sciences at the session of April 13–16, 1869.]

MANY years have passed since the suggestion was thrown out by Helmholtz, and afterwards by others, that the present volume of the sun is maintained by his internal heat, and may become less in time. Upon this hypothesis it was proposed to account for the renewal of the heat radiated from the sun, by means of the mechanical power of the sun's mass descending toward his center. Calculations made by Prof. Pierce, and I believe by others, have shown that this provides a supply of heat far greater than it is possible to attribute to the meteoric theory of Prof. Wm. Thomson, which, I understand, has been abandoned by Prof. Thomson himself as not reconcilable with astronomical facts. Some years ago the question occurred to me in connection with this theory of Helmholtz whether the entire mass of the sun might not be a mixture of transparent gases, and whether Herschel's clouds might not arise from the precipitation of some of these gases, say carbon, near the surface, with their revaporization when fallen or carried into the hotter subjacent layers of atmosphere beneath; the circulation necessary for the play of this Espian theory being of course maintained by the constant disturbance of equilibrium due to the loss of

heat by radiation from the precipitated clouds. Prof. Espy's theory of storms I first became acquainted with more than twenty years ago from lectures delivered by himself, and, original as I suppose it to be, and well supported as it is in the phenomena of terrestrial meteorology, I have long thought that Prof. Espy's labors deserve a more general recognition than they have received abroad. It is not surprising, therefore, in a time when the constitution of the sun was exciting so much discussion, that the above suggestions should have occurred to myself before I became aware of the very similar, and in the main identical, views of Prof. Faye, put forth in the Comptes Rendus. I sought to determine how far such a supposed constitution of the sun could be made to connect with the laws of the gases as known to us in terrestrial experiments at common temperatures. Some calculations based upon conjectures of the highest temperature and least density thought supposable at the sun's photosphere led me to the conclusion that it was extremely difficult, if not impossible, to make out the connection in a credible manner. Nevertheless, I mentioned my ideas to Prof. Henry, Secretary of the Smithsonian Institution, when he immediately referred me to a number of the Comptes Rendus, then recently received, containing Faye's exposition of his theory. Of course nothing is further from my purpose than to make any kind of claim to any thing in that publication. After becoming acquainted with his labors I still regarded the theory as seriously lacking, in its physical or mechanical aspect, the direct support of confirmatory observations, and even as being subject to grave difficulty in that direction. In this attitude I allowed the subject to rest until my friend Dr. Craig, in charge of the Chemical Laboratory of the Surgeon General's office, without any knowledge of Faye's memoir, or of my own suggestions previously made to Prof. Henry and another scientific friend, fell upon the same ideas of the sun's constitution, availing himself, precisely as I had done, of Espy's theory of storms. Dr. Craig's ideas were communicated to a company of scientific gentlemen early last spring, and soon after, Prof. Newcomb, of the U. S. Naval Observatory, entered into a general survey of the nebular hypothesis. These communications of Dr. Craig and Prof. Newcomb led me to enter into a renewed examination of the mechanical embarrassment under which I had believed the theory to labor. Not any longer relying on my first rough estimate based on assumed high temperatures at the photosphere, the question was now inverted. Assuming the gaseous constitution, and assuming the laws expressed in Poisson's formulæ, known to govern the constitution of gases at common temperatures and densities, what shall we find to be the temperatures and densities corresponding to the observed volume of the sun supposing

it were composed of some known gas such as hydrogen, or supposing it to be composed of such a mixture of gases as would be represented by common air. Pure hydrogen will, of course, give us the lowest temperature of all known substances, under the general hypothesis.

The question was resolved, and the results were communicated in graphical and numerical form in May or June last to two or three scientific friends, but their publication has been delayed by an unavoidable absence of several months from home.

Premising that the unit of density shall correspond to a unit of mass in the cube of the unit of length, the unit of force to the force of terrestrial gravity in the unit of mass, and the unit of pressure or elasticity in the gas to the unit of force on a surface equal to the square of the unit of length:

Let r=the distance of an element of the sun's mass from the sun's center,

t=the temperature of the element,

σt=its atmospheric subtangent, referred to the force of gravity at the earth's surface, or height of the column of homogeneous gas, whose terrestrial gravitating force would equal its elasticity,

ϱ=its density, or mass of its unit volume,

=force of terrestrial gravity in its unit volume,

$\varrho \sigma t$=its elasticity, or elastic force per unit surface,

R=the earth's radius,

M=the earth's mass,

m=the mass of the part of the sun's body contained in the concentric sphere whose radius is r,

$\frac{M r^2}{m R^2} \sigma t$= the subtangent of the gas under its actual gravitating force in the sun.

The condition of equilibrium between the gravitating force of a thin horizontal layer of gas whose thickness is dr, and the difference of elastic force between its lower and upper surfaces, is expressed by the equation,

$$d \cdot \varrho \sigma t = -\frac{m R^2}{M r^2} \varrho \, d r.$$

Under the hypothesis that the law of Mariotte and the law of Poisson prevail throughout the whole mass, and that this mass is in convective equilibrium, we have

$$\sigma = \text{a constant}, \qquad (1)$$
$$t = t_1 \varrho^{k-1},*$$

t_1 representing the value of t in the part of the mass where the density is a unit.

The theoretical difficulties which, if the supply of solar heat

* k represents the ratio of the specific heat of a gas under constant pressure to its specific heat under constant volume.

is to be kept up by the potential due to the mutual approach of the parts of the sun's mass consequent on the loss of heat by radiation, come in when we suppose a material departure from these laws of Mariotte and of Poisson at the extreme temperatures and pressures in the sun's body, or how far such difficulties intervene, will be considered further on.

By means of the constant value of σ, and the value of t given in (1), the above differential equation is transformed into

$$k\,\sigma\,t_1\,\varrho^{k-2}d\varrho = -\frac{m}{M}\,\frac{R^2}{r^2}\,d\,r,$$

the integral of which gives

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{k-1} = \frac{k-1}{k}\,\frac{R^2}{\sigma M t_1\,\varrho_0{}^{k-1}}\int_0^r \frac{m dr}{r_2}, \qquad (2)$$

in which ϱ_0 is the value of ϱ at the sun's center.

We have also

$$m = 4\pi\int_0^r \varrho r^2 dr = 4\pi\varrho_0 \int_0^r \frac{\varrho}{\varrho_0} r^2 dr. \qquad (3)$$

If now we put

$$r = \left(\frac{k\sigma M t_1}{4(k-1)R^2\pi\varrho_0{}^{2-k}}\right)^{\frac{1}{2}} x, \qquad (4)$$

we shall have

$$m = 4\pi\varrho_0\left(\frac{k\sigma M t_1}{4(k-1)R^2\pi\varrho_0{}^{2-k}}\right)^{\frac{3}{2}} \mu, \qquad (5)$$

in which

$$\mu = \int_0^x \frac{\varrho}{\varrho_0} x^2 dx, \qquad (6)$$

and equation (2) becomes

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{k-1} = \int \frac{\mu dx}{x^2}. \qquad (7)$$

In equations (6) and (7) it is plain that upon the value of k alone depends: first the form of the curve that expresses the value of $\frac{\varrho}{\varrho_0}$ for each value of x; secondly, the value of the upper limit of x corresponding to $\frac{\varrho}{\varrho_0}=0$; and thirdly, the corresponding value of μ. These limiting, or terminal, values of x and μ, cannot be found except by calculating the curve, for equations (6) and (7) seem incapable of being reduced to a complete general integral. But when these values have been found for any proposed value of k, they may be introduced once for all into equations (4) and (5), from which the values of ϱ_0, and of σt_1, are at once deduced.

I have made these calculations for two different assumed values of k, viz., $k=1{\cdot}4$, which is near the experimental value

it has in common air, and $k=1\frac{2}{3}$, which is the maximum possible value it can have in the light of Clausius' theory of the constitution of the gases. The calculation of the curve of $\frac{\varrho}{\varrho_0}$, or of $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$, begins at the sun's center where $x=0$. For the small values of x, integration by series enables us readily to deduce from equations (6) and (7) the following approximate numerical equations:

For $k=1{\cdot}4$,

$$\mu=\tfrac{1}{3}x^3-\tfrac{1}{12}x^5+\tfrac{5}{336}x^7-\tfrac{125}{5443 2}x^9+\text{ \&c.} \qquad (8)$$

$$1-\left(\frac{1}{\varrho_0}\right)^{\cdot 4}=\tfrac{1}{6}x^2-\tfrac{1}{48}x^4+\tfrac{5}{2016}x^6-\tfrac{125}{43545 6}x^8+\text{ \&c.} \qquad (9)$$

For $k=1\frac{2}{3}$,

$$\mu=\tfrac{1}{3}x^3-\tfrac{1}{20}x^5+\tfrac{1}{240}x^7-\tfrac{1}{3888}x^9+\text{ \&c.} \qquad (10)$$

$$1-\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\tfrac{1}{6}x^2-\tfrac{1}{80}x^4+\tfrac{1}{1440}x^6-\tfrac{1}{31104}x^8+\text{ \&c.} \qquad (11)$$

For larger values of x, until $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ becomes sufficiently small as there is no need of great precision in these calculations, I have merely developed the values of μ and $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ corresponding to $x=1{\cdot}1$, $x=1{\cdot}2$, $x=1{\cdot}3$, &c., by means of differences taken from the differential co-efficients at the middle of each increment of x, and for the same reason have thought it sufficient to begin with $x=1$, in equations (8) and (9) or (10) and (11). After arriving at a sufficiently small value of $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ the calculation is finished by aid of the following approximate equations also derived by integration fron (6) and (7).

$$\mu'-\mu=\frac{k-1}{k}\mu'^{\frac{1}{k-1}}x'^{2-\frac{2}{k-1}}(x'-x)^{1+\frac{1}{k-1}}(1+X) \qquad (12)$$

$$\left(\frac{\varrho}{\varrho_0}\right)^{k-1}=\frac{\mu'(x'-x)}{x'x}-\frac{(k-1)^2}{k(2k-1)}\mu'^{\frac{1}{k-1}}x'^{-\frac{2}{k-1}}(x'-x)^{2+\frac{1}{k-1}}$$
$$-\frac{(k-1)(2k^2-3k+2)}{k(2k-1)(3k-2)}\mu'^{\frac{1}{k-1}}x'^{-1-\frac{2}{k-1}}(x'-x)^{3+\frac{1}{k-1}} \qquad (13)$$

In these equations x' and μ' are the values of x and μ corresponding to $\frac{\varrho}{\varrho_0}=0$, or the upper limit of the supposed solar atmosphere, and

$$X=-\frac{k(2k-3)}{(k-1)(2k-1)}\frac{x'-x}{x'}+\frac{k(k-2)(2k-3)}{2(k-1)^2(3k-2)}\frac{(x'-x)^2}{x'^2}+\text{ \&c.}$$

$$-\frac{k-1}{2k(2k-1)}\mu' x'^{-1+\frac{1}{k-1}}(x'-x)^{1+\frac{1}{k-1}}+\text{ \&c.}$$

With the values of x' and μ' determined, using r' and m' to express in like manner the corresponding values of r and m at the upper limit of the theoretical atmosphere, we find from equations (4) and (5)

$$\varrho_0=\frac{m'x'^3}{4\pi\mu' r'^3}, \tag{14}$$

$$\sigma t_1=\frac{4\pi(k-1)R^2r'^2\varrho_0^{2-k}}{kMx'^2},$$

and by equation (1), $$\sigma t=\frac{4\pi(k-1)R^2r'^2\varrho_0}{kMx'^2}\left(\frac{\varrho}{\varrho_0}\right)^{k-1}, \tag{15}$$

$$=\frac{k-1}{k}\frac{m'R^2x'}{\mu' Mr'}\left(\frac{\varrho}{\varrho_0}\right)^{k-1} \tag{16}$$

A glance at equation (7) will show that $\frac{\mu'(x'-x)}{x'x}$, equation (13), or $\frac{\mu'}{x'}\frac{r'-r}{r}$ may be taken equal to $\left(\frac{\varrho}{\varrho_0}\right)^{k-1}$ throughout the considerable upper part of the volume of the hypothetic gaseous body in which $1-\frac{\mu}{\mu'}$, or $1-\frac{m}{m'}$, is sufficiently small to be neglected. This substitution in the last equation gives

$$\sigma t=\frac{k-1}{k}\frac{m'R^2}{Mrr'}(r'-r), \text{ nearly,} \tag{17}$$

and also $$\varrho=\left(\frac{\mu'}{x'}\right)^{\frac{1}{k-1}}\varrho_0\left(\frac{r'-r}{r}\right)^{\frac{1}{k-1}} \text{ nearly,}$$

$$=\frac{1}{4\pi}\mu'^{-1+\frac{1}{k-1}}x'^{3-\frac{1}{k-1}}\frac{m'}{r'^3}\left(\frac{r'-r}{r}\right)^{\frac{1}{k-1}} \tag{18}$$

Now the mechanical equivalent of the heat in the mass ϱ of a cubic unit in volume of any perfect gas whose atmospheric subtangent is σt, is $\frac{1}{k-1}\varrho\cdot\sigma t$, and the mechanical equivalent of the heat that it would give out, in being cooled down under constant pressure to absolute zero, is $\frac{k}{k-1}\varrho\cdot\sigma t$. If the density ϱ is taken in units of the density of water, and the unit of

length be the foot, this expression is multiplied by $62\frac{1}{2}$ to give for the mechanical equivalent in foot pounds

$$62\tfrac{1}{2}\frac{k}{k-1}\varrho\cdot\sigma t=\frac{62\tfrac{1}{2}}{4\pi}\mu'^{-1+\frac{1}{k-1}}x'^{3-\frac{1}{k-1}}\frac{m'^2R^2}{Mr'^4}\left(\frac{r'-r}{r}\right)^{1+\frac{1}{k-1}} \qquad (19)$$

The mechanical equivalent $\frac{1}{k-1}\varrho\cdot\sigma t$, of the heat in the mass ϱ, viewed in the light of Clausius' mechanical theory of the gases, includes the motions of the separate atoms of each supposed compound molecule relatively to each other, as well as the motion of translation which each compound molecule makes in a straight path through free space till it impinges upon another compound molecule. If we wish to find the mechanical equivalent which would be due to this motion of translation alone, we must put $k=1\frac{2}{3}$ in the factor $\frac{1}{k-1}$ by which $\varrho\cdot\sigma t$ is multiplied, and this gives $\frac{3}{2}\varrho\cdot\sigma t$. To find from this the mean of the squares of the velocities of translation of the compound molecules, we divide by the mass ϱ, and, if the foot be the unit of length, multiply by 64·3, whence we have for the velocity found by taking the square root of this mean of the squares

$$8{\cdot}02\sqrt{\tfrac{3}{2}\sigma t}=8{\cdot}02\left(\frac{3}{2}\frac{k-1}{k}\frac{m'R^2x'}{\mu'Mr'}\right)^{\frac{1}{2}}\left(\frac{\varrho}{\varrho_0}\right)^{\frac{k-1}{2}} \qquad (20)$$

Determination of the curve of density for $k=1{\cdot}4$.—Beginning with $x=1$, in equations (8) and (9), we find $\mu=\cdot2626$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{4}{10}}=\cdot8520$. Developing the values of μ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{4}{10}}$ for $x=1{\cdot}1$, $x=1{\cdot}2$, &c., by means of differences we arrive at the values $\mu=2{\cdot}145$ and $\left(\frac{\varrho}{\varrho^0}\right)^{\frac{4}{10}}=\cdot1378$ when $x=4{\cdot}0$. Putting these values into equations (12) and (13) we find

$$x'=5{\cdot}355,\quad \mu'=2{\cdot}188.$$

If we now allow $\frac{1}{2\frac{1}{2}}$d of the radius of the photosphere, or about 20,000 miles, for the height of the theoretic upper limit of the solar atmosphere above the photosphere, and if we take the mean specific gravity of the earth's mass at $5\frac{1}{2}$, and the mean specific gravity of the sun within the photosphere at $\frac{1}{4}$ that of the earth, as it is known to be, these values of x' and μ' give us in equation (14)

$$\varrho_0=28{\cdot}16,$$

so that the density of the sun's mass at the center would be nearly one-third greater than that of the metal platinum.

Curve of density for $k=1\frac{2}{3}$.—For this value of k the numerical coefficients in equations (8) and (9) are replaced by those in (10)

and (11). Otherwise, the same process employed with the value $k=1{\cdot}4$, gives, starting with $x=1$, $\mu=\cdot2875$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\cdot8452$, and developing for $x=1{\cdot}1$, $x=1{\cdot}2$, &c., brings us to $\mu=2{\cdot}557$ and $\left(\frac{\varrho}{\varrho_0}\right)^{\frac{2}{3}}=\cdot1591$, for $x=3{\cdot}0$, and finally gives us

$$x'=3{\cdot}656,\ \mu'=2{\cdot}741,$$

and if we now assume the same height as before for the theoretic upper limit of the sun's atmosphere, instead of $\varrho_0=28{\cdot}16$, we find

$$\varrho_0=7{\cdot}11.$$

The new curve of density is found in the same way as the first, and is presented to the eye in the diagram in comparison with it. In the upper part of both curves the scale of density is increased ten fold, and it is, in part only, evident to the eye how immensely different, for the two values of k, becomes the density in the upper parts of the sun's mass. It appears to the eye only in part because the ratio of the two densities multiplies itself rapidly in approaching the upper limit of the atmosphere.

The above was communicated in writing as here given, to the Academy at its late session.* The draft of the following, and a part of the details of its substance, have been prepared since.

Equation (20) gives in feet the square root of the mean square of velocity of translation of molecules ($8{\cdot}02\sqrt{\frac{3}{2}\sigma t}$). At the sun's center we find this would be 331 miles per second for the curve of density corresponding to $k=1\frac{2}{3}$, and 380 miles per second for the curve of density corresponding to $k=1{\cdot}4$.

In 1838 Pouillet, following the law of heat radiation given by Dulong and Petit, estimated the temperature of the radiating surface of the sun, from observations by himself of the quantity of heat it emits, at from 1461° C. to 1761° C. Herschel, from Pouillet's observations, and his own made at the Cape of Good Hope about the same time, adopts, after allowing one-third for the absorption of our atmosphere, forty feet as the thickness of ice that would be melted per minute at the sun's sur-

* I desire here to state that the formulæ which show the relation between the temperature, the pressure, the density, and the depth below the upper limit of the atmosphere, so far as they apply to the upper part of the sun's body, were independently pointed out by Prof. Peirce, in a very interesting paper which that distinguished physicist read before the Academy at the same session, and prior to the presentation of this paper. Also to recall a fact which I first learned from Prof. Peirce's mention of it to the Academy, viz. that Prof. Henry long ago threw out the idea of the atmospheric condition to which Prof. Thomson has more recently given the term convective equilibrium, viz., such that any portion of the air, on being conveyed into any new layer above or below, would find itself reduced, by its expansion or compression, to the temperature of the new layer.

face. The temperature of the radiating surface calculated from this datum by the formula of Dulong and Petit, and with the co-efficient of radiation found by Prof. W. Hopkins for sandstone, the smallest co-efficient he found, is 1550° C. or 2820° Fah. But then the solar radiation is many thousands of times greater than the greatest in Dulong and Petit's experiments, so that these calculations of the temperature of the sun's photosphere have little weight notwithstanding the simplicity and accuracy with which the formula represents the experiments from which it was derived. Nothing authorizes us to accept the formula as more than an empirical one. It seems desirable that experiments similar to those of Dulong and Petit should be made on the rate of cooling of intensely heated bodies, such as balls of platinum not too large. By placing the heated ball in the center of a hollow spherical jacket of water, either flowing or in an unchanged mass, the quantities of heat radiated in successive equal spaces of time will be determined, and the corresponding differences of temperature in the heated ball can at least be estimated with whatever probability we may rely on our knowledge of the specific heat of its material. At present the best means we have of forming any judgment of the probable temperature of the source of the sun's radiation, is perhaps to be found in a comparison between the effects of the hydro-oxygen blowpipe, and the recorded effects of Parker's great burning lens. I am not aware that this method has before been resorted to.

If the angle of aperture at the focus of a burning lens, or combination of lenses, be called $2a$, the radiation received by a small flat surface at the focus will be $\sin^2 a$, if a unit be taken to represent the radiation the same small flat surface would receive just at the sun's surface. Parker's lens, with the small lens added, had, at the focus so formed, an angle of aperture of about 47°. A small flat surface at its focus would therefore receive about one-sixth the radiation that it would just at the sun, making no allowance for absorption by the atmospheres of the earth and sun and rays lost in transmission through the lenses. Pouillet, from the experiments already alluded to made by himself, found his atmosphere in fine weather transmitted, of the sun's heat rays, about the fraction $\frac{4}{5}$ raised to a power whose exponent is the secant of the sun's zenith distance. This, of course, leaves out of view the heat rays of low intensity which are totally absorbed by the atmosphere. He also concluded from comparison with other experiments of his own with a moderately large burning glass, that that glass transmitted $\frac{7}{8}$ of the heat rays incident on it. If we assume the same fraction for each of the two lenses of Parker's com-

bination, and assume further that the sun's zenith distance did not exceed 48° in the experiments made with it, we find for the fractional multiplier expressing the part of the sun's heat radiation which arrived at the focus unintercepted, $(\frac{4}{5})^{1\cdot 1 2}(\frac{7}{8})^2 = \cdot 55$. Hence the radiation actually received by a small flat surface at the focus was ·09, or about one-eleventh, of what it would receive just at the sun. The heat so received by any body so placed in the focus, must, after the body has acquired its highest temperature, be emitted from it at the same rate. The heat so emitted will consist: first, of heat radiated into that part of space toward which the radiating surface of the body looks; secondly, of heat carried of by convection of the air; thirdly, of heat conducted away by the body supporting the body subjected to experiment; fourthly, of heat rays, if any, reflected, and not absorbed, by the body subjected to experiment. Assuming it as a reasonable conjecture that full half of all this* consists of heat *radiated* into the single *hemisphere* looking upon a flat surface, we may conclude that the body, at its highest acquired temperature, radiated not less than $\frac{1}{2\frac{1}{5}}$th as much heat as is radiated by an equal extent of surface of the sun's photosphere, over and above such part of that radiation as may be intercepted by the sun's atmosphere, and such rays of low intensity as are *totally* absorbed by our own atmosphere, the whole of which apparently cannot be great. No allowance seems necessary for the chromatic and spherical dispersion of the lenses, since the diameter of the focus is stated at half an inch, while the true diameter of the sun's image would be not less than one-third of an inch.

Now we are not without the means of forming a probable approximate estimate of this temperature at which the radiation becomes $\frac{1}{2\frac{1}{5}}$th, more or less, of that of the sun's photosphere. We are told that in the focus of Parker's compound lens 10 grains of very pure lime ("white rhomboidal spar") were melted in 60 seconds. We may presume that in that length of time the temperature of the lime, after parting with its carbonic acid, made a near approximation to the maximum at which it would be stationary, a presumption confirmed by the period of 75 seconds said to have been occupied in the fusion of 10 grains of carnelian, and by the considerable period of 45 seconds for the fusion of a topaz of only 3 grains, and 25 seconds for an oriental emerald of but 2 grains, and in fact sufficiently

* As to the heat carried off by convection of the air, if its quantity be calculated by the formula given by Dulong and Petit for that purpose, it comes out utterly insignificant in comparison with the heat received from the burning glass. The conjectural allowance of $\frac{4}{5}$ths in all, of this, is likely, therefore, to be much too large. Not much reliance, indeed, can be placed upon the formula here mentioned, at such a temperature as 4000° Fah., yet, as by it the convection is taken proportional to the 1·233 power of the difference of temperature, it seems unlikely that it gives a quantity very many fold less than the truth.

proved, it would seem, by observing that the heat we have estimated to fall at the focus, upon a flat surface, would suffice, if retained, to raise the temperature of a quarter of an inch thick of lime 4000° Fah. in 5 seconds. If, then, we may take the temperature *maintained* at the focus of Parker's lens to have been at the melting point of lime, we may conclude that it is also not far from the temperature given by the hydro-oxygen blowpipe. Dr. Hare, who was the first inventor of this instrument, and the discoverer of its great power, melted down, by its means, in partial fusion, a very small stick of lime cut on a lump of that material, which we understand to have been a very pure specimen. Burning glass and blowpipe seem each to have been near the limit of its power in this apparently common effect. But Deville found the temperature produced by the combination of hydrogen and oxygen under the atmospheric pressure to be 2500° Cent. As the lime in the heated blast would radiate rapidly, its temperature must have been lower than that of combined hydrogen and oxygen, and I have called it 2220° Cent. or 4000° Fah.

The formula of Dulong and Petit, with the co-efficient found by Hopkins, as already mentioned, gives for the quantity of heat radiated in one minute by a square foot of surface of a body whose temperature is $\theta+t$ centigrade, into a chamber whose temperature is θ centigrade, when expressed with the unit employed by Hopkins,

$$8{\cdot}377\,(1{\cdot}0077)^{\theta}\,[(1{\cdot}0077)^{t}-1].$$

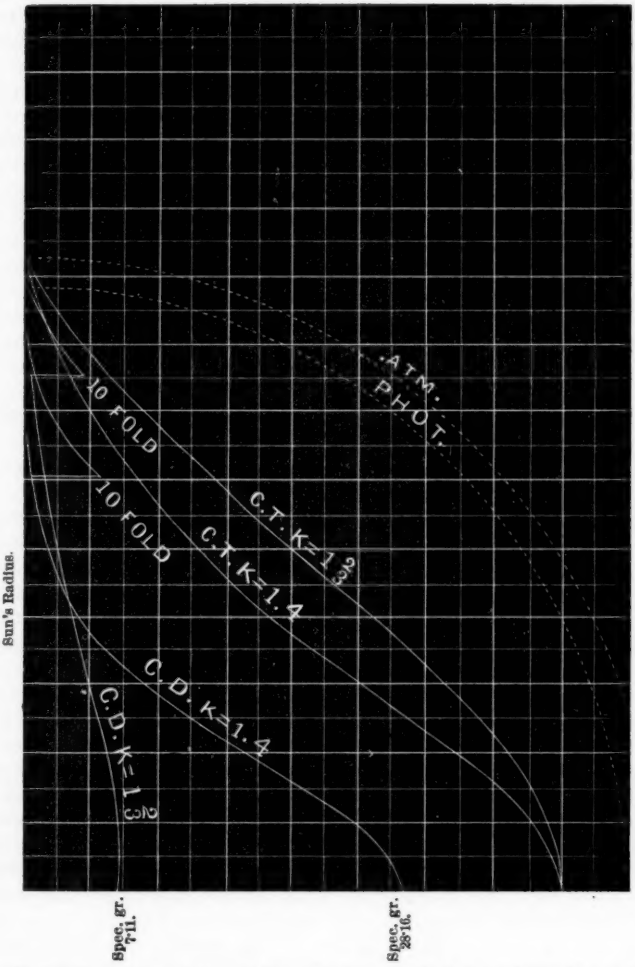
It will be convenient, and, in the discussion of the high temperatures with which we are concerned, will involve no sensible error, to use the hypothesis that the space around the radiating body is at the temperature of 0° C. and the formula for the radiation then becomes,

$$8{\cdot}377\,[(1{\cdot}0077)^{t}-1]. \qquad (21)$$

The unit used by Hopkins, in the formula here given, is the quantity of heat that will raise the temperature of 1000 grains of water 1° centigrade. Expressed by the same unit, the quantity adopted by Sir J. Herschel as the amount of the sun's radiation, viz. that which would melt 40 feet thick of ice in a minute (at the sun's surface), is 1,280,000. The $\frac{1}{20}$th of this, or 64,000, expresses, therefore, the quantity which we have estimated the lime under Parker's lens to have radiated, per square foot of its surface, at its estimated temperature of 4000° Fah. If now we calculate its temperature by the above formula, from the estimated radiation, the result is 1166° Cent. or 2130° Fah. This is manifestly much below the real temperature, and so far below that there can be no doubt the formula of Dulong and Petit has failed at the melting point of lime. If

instead of the co-efficient 8·377 we had used the larger co-efficient 12·808 which Hopkins gives for unpolished limestone, the formula would have been reduced only 53° Cent. It best suits the direction of our inquiry to use the smallest co-efficient which Hopkins' experiments gave, since we are seeking the *highest* temperature which can be plausibly deduced from the sun's radiation. For ease of expression, the curve which we will imagine for representing the actual relation of radiation to temperature, the horizontal ordinate standing for the temperature and the vertical ordinate for the radiation corresponding thereto, may be called the curve of radiation. The course of this curve from the freezing point of water to a point somewhat below the boiling point of mercury is correctly marked out to us by the formula. Beyond that we have but the rough approximation which we can get by means of the above comparison, to the single point of the curve where the radiation is $\frac{1}{2}\frac{1}{5}$th that of the sun's photosphere. The attempt, from these data, to extend the curve till it reaches the full radiation of that photosphere, must be mainly conjectural. As a basis for the most plausible conjecture I am able to make let us assume: first, that the upward concavity of the curve of radiation, which increases very rapidly with the temperature as far as the curve follows the formula of Dulong and Petit, is at no temperature greater than that formula would give it at the same temperature; secondly, that the curve of radiation is nowhere convex upward. If, then, we set out from these two conjectural assumptions—of the degree of probability of which each one must form his own impression—the greatest temperature the sun's photosphere could have consistently with the radiation of 64,000 at the temperature of 4000° Fah., is found by drawing through the point representing that radiation and that temperature a straight line tangent to the curve of the formula. The line so drawn would cross the real curve of radiation in a greater or less angle at the radiation of 64,000 and temperature of 4000° Fah., and at higher temperatures would fall more or less *below* that curve, and its intersection with the sun's radiation of 1,280,000 would be at a temperature greater than that of the curve, that is to say, greater than the temperature of the sun's photosphere. This greater temperature is 55,450° Fah.

A different train of conjecture led me at first to assume a temperature of 54,000° Fah., and this last number I will here retain since it has been already used as the basis of some of the calculations we now proceed to give. It must be here recollected that we are discussing the question of *clouds* of *solid* or at least *fluid* particles floating in non-radiant gas, and constituting the sun's photosphere. If the amount of *radiation*



Explanation.—ATM., Assumed theoretic upper limit of atmosphere; PHOT.. Photosphere; C.T.K $=1\frac{5}{7}$, Arbitrary Curve of temperature for $k=1\frac{5}{7}$; C.T.K.$=1{\cdot}4$, Arbitrary Curve of temperature for $k=1{\cdot}4$; C.D.K.$=1{\cdot}4$, Absolute Curve of density for $k=1{\cdot}4$; C.D.K.$=1\frac{5}{7}$, Absolute density for $k=1\frac{5}{7}$.

would lead us to limit the temperature of such clouds of solids or fluids, so also it seems difficult to credit the *existence* in the solid or fluid form, at a higher temperature than 54,000° Fah. of any substance that we know of.

If then we suppose a temperature of 54,000° Fah., what would be the density of that layer of the hypothetic gaseous body which has that temperature, and what length of time would be required, at the observed rate of solar radiation, for the emission of all the heat that a foot thick of that layer would give out in cooling down under pressure to absolute zero? The latter question depends on the mechanical equivalent of this heat for a cubic foot of the layer of gas, and the two questions, together with that of the depth at which the layer would be situated below the theoretic upper limit of the atmosphere, are answered by equations (17), (18), and (19), provided we knew the value of k and the value of σ in the body of gas. The less the atomic weight of the gas the greater the value of σ, and the greater the density of the layer of 54000° Fah. and the greater the quantity of heat which a cubic foot of it would give out in cooling down. I therefore base the first calculation on hydrogen as it is known to us. The value of σ is in that case about 800 feet, and the value of k about 1·4, nearly the same as in common air. These values would give for the layer of 54000° Fah. a specific gravity about ·00000095 that of water, or about one 90th that of hydrogen gas at common temperature and pressure, and the mechanical equivalent of the heat that a cubic foot of the layer would give out in cooling down under pressure to absolute zero would be only about 9000 foot pounds, whereas the mechanical equivalent of the heat radiated by one square foot of the sun's surface in one minute is about 254,000,000 foot pounds. The heat emitted each minute would, therefore, be fully half of all that a layer ten miles thick would give out in cooling down to zero, and a circulation that would dispose of volumes of cooled atmosphere at such a rate seems inconceivable.

It may possibly appear to some minds that the difficulty presented by this aspect of the case will vanish if we suppose the photosphere, or its cloudy particles, to be maintained by radiation at a temperature to almost any extent lower than that of convective equilibrium. This would enable us to place the theater of operations in a lower and denser layer of atmosphere, but the supposition seems to me difficult to realize unless, as the hot gases rise from beneath, precipitation could commence at a temperature many times higher than the 54000° Fah. which we have estimated for the upper visible surface of the clouds, and this, as before intimated, seems to me itself extremely improbable.

I may mention here that my friend Dr. Craig, in an unpublished paper, following the hint thrown out by Frankland, is disposed to favor the idea that the sun's radiation may be the radiation of hot gases instead of clouds. At present I shall offer no opinion on that point one way or the other, but will only state it as my impression that if the theory of precipitated clouds, as above presented, is the true one, something quite unlike our present experimental knowledge, or at least much beyond it, is needed to make it intelligible.

The first hypothesis which offers itself in an attempt to make the theory rational is suggested by one point in Clausius' theory of the constitution of the gases, already alluded to. In forming his theory Clausius found that the known specific heats of the gases are all much too great for free simple atoms impinging on one another, and he therefore introduced the hypothesis of compound molecules, each compound molecule being a system of atoms oscillating among each other under forces of mutual attraction. Now if this were accepted as the actual constitution of the gases it is of course easy enough to conceive that in the fierce collisions of these compound molecules with each other at the temperatures supposed to exist in the sun's body, their component atoms might be torn asunder, and might thenceforth move as free simple molecules. In this case, still retaining the hypothesis of Clausius' theory, that the average length of the path described by each between collisions is large compared with the diameter of the sphere of effective attraction or repulsion of atom for atom, the value of k would reach its maximum of $1\frac{2}{3}$. Experiment has not shown us any gas in this condition, and for the present it is hypothetical. Even in hydrogen the value of k does not materially, if any, exceed the value of 1·4 which it has in air. But if it were found that the hydrogen molecule is compound, and that in the body of the sun the heat splits this molecule into two equal simple atoms, and in fact that all the matter in the sun's body is split into simple free atoms equally as small, then, while the value of k would be $1\frac{2}{3}$, the value of σ would be about 1600 feet. If with these values we repeat the calculation of the density of the layer of 54000° Fah. we find its specific gravity to be 0·000363 of that of water, or 4·35 times that of hydrogen gas at common temperature and pressure and in its known condition, or 8·7 times that which the hydrogen in the hypothetic condition would have if it retained that condition at common temperature and pressure. We find also that the mechanical equivalent of all the heat that a cubic foot of the layer would give out in cooling down, under pressure, to zero, would be no less than 13,500,000 foot pounds. Instead, therefore, of a layer ten miles thick, it would now require only a thickness of 38 feet

to give out, in cooling down to zero, twice the heat emitted by the sun in one minute. It will be seen, (equations (17) and (19)), that this thickness, retaining the constant value $k=1\frac{2}{3}$, would diminish with the $2\frac{1}{2}$ power of the masses of the atoms into which the sun's body is hypothetically resolved (the reciprocal of the value of σ), and I leave each to form his own impression how far this view leads towards verisimilitude.

It is important to add that the depth of the layer of 54000° Fah. below the theoretic upper limit of atmosphere, when calculated with value $k=1{\cdot}4$, $\sigma=800$ feet, comes out only 1107 miles, and with the values $k=1\frac{2}{3}$ and $\sigma=1600$ feet only 1581 miles. This calculation of the depth, unlike the other results above, may be said to be independent of the question of the constitution of the sun's interior mass. It is alike difficult, on any plausible hypothesis, to reconcile a temperature no higher than 54000° Fah. with any perceptible atmosphere extending many thousand miles above, and yet no less an authority than Prof. Peirce has assigned a hundred thousand miles as the height of the solar atmosphere above the photosphere, at the same time, however, pointing out the enormous temperature which, under convective equilibrium, this would imply at the level of the photosphere. But all are not yet agreed that the appearances seen at such distances from the sun are proof of the existence of a true atmosphere there. It will be seen that the numbers I give above were obtained from a first hypothesis of an atmospheric limit 20,000 miles above the photosphere, but for the purpose of this paper it is of no consequence to repeat the calculation from a different limit.

It is, I believe, recognized on theoretical grounds that in an atmosphere containing a mixture of gases of unequal density the lighter gases might be expected to diffuse in greater proportion into the higher parts of the atmosphere and the heavier gases into the lower parts. But perhaps the supposed circulation which the emission of heat maintains within the photosphere must renew mixture at a rate sufficient to mask the rate which theory would assign for diffusion. I have not attempted a theoretic comparison between these two tendences. It will suffice here to repeat that the above numerical results, so far as they may be thought to give countenance to the theory in its mechanical aspect, require that the entire inner mass of the sun shall have, at a mean, (in the supposed state of dissociation), the very small atomic weight specified. We may notice in this connection the uniform proportion of oxygen and nitrogen gases in our atmosphere at the height of four miles or more at which the analysis has been made. Without having gone into a critical examination of the question, I suppose that at that height the proportion of oxygen which the theory of diffusive equili-

brium would assign is notably diminished, and that it would be found that the circulation of the air is sufficiently active to mask the theoretic rate of diffusion.

The second hypothesis which might offer itself in an attempt to make the theory rational, but which a very little reflection is, I think, sufficient to set aside, is that which would modify Clausius' theory of the gases by assuming that in the sun's body the average length of the excursion made by each molecule between two consecutive collisions, becomes very short compared with the radius of the sphere of repulsion of molecule for molecule, and with the average distance of their centers at nearest approach. This way of harmonizing the actual volume of the sun with such a temperature as 54000° Fah. in the photosphere, and with the smallest density which we can credit in the photosphere, would involve the consequence that the existing density of almost the entire mass of the sun is very nearly uniform and at its maximum possible, or at all events that any further sensible amount of collapse must be productive of but a very small amount, comparatively, of renewed supplies of heat, for the obvious reason that this hyphothesis carries with it almost the entire neutralization of the force of gravity by the forces of molecular repulsion. In like manner it involves the consequence that in any such small contraction of the photosphere as can have taken place within the history of total eclipses, it is but a very small fraction of the sun's mass, near its surface, that can have taken part in the collapse to any thing like a proportionate extent. Hence it also extremely restricts the period during which we could suppose the sun to have existed under anything like its present visible magnitude in the past, consistently with the production in the way supposed of the supplies of heat it has been sending out. Another thing involved in this second hypothesis is the fact which Prof. Peirce has pointed out to the Academy, viz.: that the existing molecular repulsion in the sun's body would immensely exceed such as would be indicated by the modulus of elasticity of any form of matter known to us.

In conclusion, I do not mean to say that there is any *invincible logical exclusion* of any law of the action of gases different from what is specified or alluded to above. I only mean that, so far as I can see, any theory of heat which is based *simply* and *solely* upon *molecular attraction* and *repulsion* dependent on molecular distance alone, cannot in its application to the sun, escape from the conditions indicated in this paper. It is certainly not absurd to imagine heat to be an agent of some kind so constituted that it cannot be thus represented by the sole conditions of motion and of molecular attraction and repulsion, but yet so constituted that in its effects upon matter it follows

the conditions of mechanical equivalency as defined by Joule. In fact, such exceptional cases as the expansion of water in freezing seem to favor such a view, though the range of that phenomenon is very limited. One way of forming a mechanical representation of such a constitution would be by associating molecular motion with the mechanical powers, either with or without molecular attraction or repulsion; the manner in which the imagined mechanical power (or link) attaches itself to the molecules which it connects—so as to make their motion determine their mutual approach or recession or change of relative direction—being dependent on the existing motions and other conditions in such a way as to produce the observed phenomena. The possibility of such a mechanical *representation* is sufficient to show that such a supposed *constitution* is not *logically excluded*, but to accept such a mechanical representation as a physical fact is quite another matter, and, as it seems to me, a very difficult one. Of course this difficulty does not present itself when we suppose that heat is not motion.

ART. X.—*On the mode of observing the coming Transits of Venus;* by SIMON NEWCOMB.*

Transits of Venus over the disk of the sun have more than any other celestial phenomena occupied the attention and called forth the energies of the astronomical world. In the last century they furnished the only means known of learning the distance of the sun with an approach to accuracy, and were therefore looked for with an interest corresponding to the importance of this element. Although other methods of arriving at this knowledge with about equal accuracy are now known, the rarity of the phenomenon in question insures for it an amount of attention which no other system of observation can command. As the rival method, that of observations of Mars at favorable times, requires, equally with this, the general coöperation of astronomers, the power of securing this coöperation does in itself give the transits of Venus an advantage they would not otherwise possess.

Although the next transit does not occur for four years, the preliminary arrangements for its observation are already being made by the governments and scientific organizations of Europe. It is not likely that our government will be backward in furnishing the means to enable its astronomers to take part in this work. The principal dangers are, I apprehend, those of setting out with insufficient preparation, with unmatured plans

* Read before the National Academy of Sciences, April 13, 1870.

of observation, and without a good system of coöperation among the several parties. For this reason I beg leave to call the attention of the Academy to a discussion of the measures by which we may hope for an accurate result.

In planning determinations of the solar parallax from the transits of Venus, it has until lately been the custom to depend entirely upon observations of the internal contact of the limbs of the sun and planet, as proposed by Halley. It is a little remarkable that while astronomical observations in general have attained a degree of accuracy wholly unthought of in the time of Halley, this particular observation has never been made with a precision at all approaching that which Halley believed that he himself had actually attained. In his paper he states that he was sure of the time of the internal contact of Mercury and and the sun within a second.* The latest observations of a transit of Mercury, made in November, 1868, are, as we shall presently see, uncertain by several seconds. It is also well known that the observations of the last transit of Venus, that of June, 1769, failed to fix the solar parallax with the certainty which was looked for, the result of the standard discussion being now known to be erroneous by one-thirtieth of its entire amount.

The discrepancies which have always been found in the class of observations referred to, when the results of different observers have been compared, have been generally attributed to the effect of irradiation. The phenomenon of irradiation presents itself in this form: When we view a bright body, projected upon a dark ground, the apparent contour of the bright body projects beyond its actual contour. It may be generalized as follows:—A lucid point, however viewed, presents itself to the sense, not as a mathematical point, but as a disc of appreciable extent, and, usually, of irregular outline. But, for our present purposes the form of the disc may be considered circular. Its outline is necessarily quite indefinite, and its magnitude increases with the brilliancy of the point. A bright body, being composed of an infinity of lucid points, its apparent enlargement is an evident result of this law.

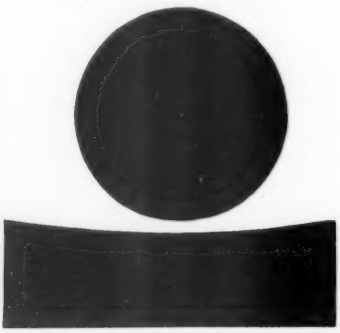
The following diagrams show the effect of this law upon the time of internal contact of a planet with the disk of the sun. The planet being supposed to approach the solar disk, figure 1 shows the geometrical form of a portion of the apparent surface of the sun, or the phenomenon as it would be if there were no irradiation immediately before the moment of internal contact. Figure 2 shows the corresponding appearance immediately after the contact. To indicate the effect of irradiation, or to show the phenomenon as it will actually appear on the theory of irra-

* Philosophical Transactions, No. 348, p. 454.

diation, we have only to draw an infinity of minute circles, one from each point of the sun's disk visible around the planet.

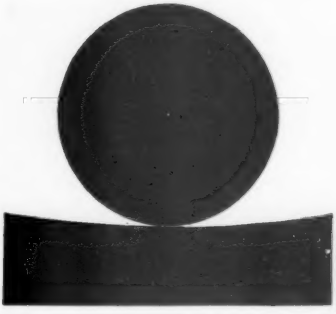
1.

1*a*.

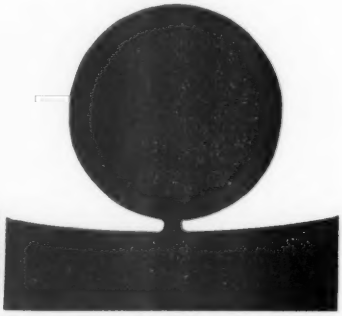


The effect of this is shown in the figures 1*a* and 2*a*. The exceedingly thin thread shown in figure 1 is thus thickened as

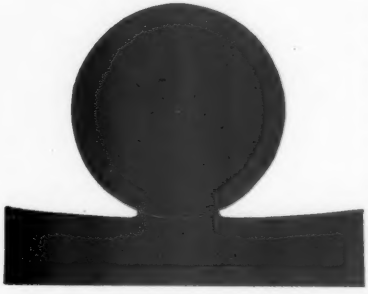
2.



2*a*.



3.



in 1*a*, and the sharp cusps of figure 2 are rounded off as shown in figure 2*a*. The apparent radius of the planet is diminished by an amount equal to the radius of the circle of irradiation, and the radius of the sun is increased by the same amount. Comparing figures 1*a* and 2*a*, it will be seen that the moment of internal contact is marked by the apparent formation of a ligament, or "black drop," between the limbs of the sun and the planet. This formation is of so marked a character that it

has been generally supposed there could be little doubt of the moment of its occurrence. The remarks of the observers have given color to this supposition, the black drop being generally described as appearing suddenly at a definite moment.

Examining figure 2*a*, it will be seen that the planet still appears entirely within the disk of the sun. The geometrical circle which bounds the latter, and that which bounds the planet, instead of touching, are separated by an amount equal to double the irradiation. If the observer waits until the planet has moved over this space, the two circles will appear in that position in which they would touch if completed. This phase commonly called "apparent contact," is shown in fig. 3. Neither circle is visible at the point of tangency, the black drop, now greatly widened and flattened, extending on both sides this point. The estimate of the moment of contact must therefore be very rough, the means of estimating being far less accurate than those afforded by a common filar micrometer. In the actual case the eye has to continue the two circles to the point of contact by estimation through a distance depending on the amount of irradiation, while measures with a micrometer are made by actual contact of a wire with a disk. Such estimates have, therefore, been generally rejected by investigators, not only from their necessary inaccuracy, but because the time of "apparent contact" depends upon the amount of irradiation, which varies with the observer and the telescope. If there is no irradiation at all, the time of apparent contact and that of true contact will be the same, as shown in figure 2; while, when the cusps are enlarged by irradiation, apparent contact will not occur until the planet has moved through a space equal to double the irradiation.

Let us return to the phenomena at actual contact. According to the theory as it has been presented, the formation or rupture of the black ligament connecting the dark body of Venus with the dark ground of the sky is a well-marked phenomenon, occurring at the moment of true internal contact. This was, I believe, the received theory until Wolf and André made their experiments on artificial transits in the autumn and winter of 1868 and 1869. They announced, as a result of these experiments, that the formation of the ligament was not cotemporaneous with the occurence of internal contact, but followed it at the ingress of the planet, and preceded it at egress. In other words, it appeared while the thread of light was still complete. They furthermore announced that with a good telescope the ligament did not appear at all, but the thread of light between Venus and the dark sky broke off by becoming indefinitely thin.

The result is not difficult to account for. Irradiation has already been described as a spreading of the light emitted from

each point of the surface viewed, so that every such point appears as a small circle. The obvious effect of this spreading is a dilution of the light emitted by a luminous thread or point whenever the diameter of the thread is less than that of the circle of irradiation. In consequence of this dilution the thread may be invisible while it is really of sensible thickness, a given amount of light producing a greater effect on the eye the more it is concentrated. Since the thread of light must seem to break when it becomes invisible at its thinnest point, the formation or rupture of the thread marks, not the moment of actual contact, but the moment at which the thread of light becomes so thick as to be visible, or so thin as to be invisible. The greater the irradiation, and the worse the definition, the thicker will be the thread at this moment.

An interesting observation illustrative of this point was made by Liais at Rio Janeiro during the transit of Mercury of November 1, 1868. He had two telescopes, one much smaller than the other. He watched the planet in the small one till it seemed to touch the disk of the sun. Then looking into the large one he saw a thread of light distinctly between the planet and the sun, and they did not really touch until several seconds later.*

Reference to the figures will make it clear that there is no generic difference between the phenomenon commonly called the rupture of the black drop and that of the formation of the thread of light. If the bright cusps are much rounded, as in figure 2*a*, the appearance between them is necessarily that of a drop, while if they are seen in their true sharpness, as in figure 1, the form of the drop will not appear. It has been shown that with different instruments the phenomenon of contact may exhibit every gradation between these extremes. The only well defined phenomenon which all can see is the meeting of the bright cusps and the consequent formation of the thread of light at ingress and the rupture of the thread at egress.

To recapitulate our conclusions—

1. The moment of observed internal contact at ingress is that at which the thread of light between Venus and the sun becomes thick enough to be visible.

2. The least visible thickness varies with the observer and the instrument, and, perhaps, with the state of the atmosphere.

3. The apparent initial thickness of the thread varies with the irradiation of the telescope.

Two questions are now to be discussed. The observed times varying with the observer and the instrument, we must know how wide the variation may be. If it be wide enough to ren-

* Astronomische Nachrichten, B. 73, S. 209.

der uncertain the results of observation, we shall inquire how its injurious effects may be obviated.

The first question can be decided only by comparison of the observations of different observers upon one and the same phenomenon. For such comparison I shall select the observations of the egress of Mercury on the occasion of its last transit over the disk of the sun. This selection is made for the reason that this egress was observed by a great number of experienced observers with the best instruments, while former transits, whether of Venus or Mercury, have been observed less extensively or at a time when practical astronomy was far from its present state of perfection, and that the transit in question would therefore furnish much better data of judging what we might expect in future observations. The comparison was made in the following way: I selected from the "Astronomische Nachrichten," the "Monthly Notices of the Royal Astronomical Society," and the "Comptes Rendus" all the observations of internal contact at egress which there was reason to believe related to the breaking of the thread of light, and which were made at stations of known longitude. Each observation was then reduced to Greenwich time and to the center of the earth.

The results are exhibited in the following table. The third column of the table shows the corrected reduced times of contact. Next is given the aperture of the telescope, and the magnifying power employed in the observation. Lastly, we present a classification of the phenomena of contact as described by the observers.

The letters V. C., (vanishing contact,) indicate that the thread of light broke by becoming indefinitely thin, so that there was no irradiation or distortion of the planet.

The letters B. D. (black drop) indicate the appearance of the ligament, as shown in fig. 2*a*.

R. R. Indicates that the time is that of the rupture of the thread, but that it cannot be inferred from the statements of the observer whether the thread had or had not a sensible thickness immediately before breaking.

A. C. Signifies an apparent contact, as shown in fig. 3. When there is no black drop the letters V. C. and A. C. have the same meaning. The letters are therefore employed only when there is reason to believe from the remarks of the observer that the cusps were rounded.

(Inst.) indicates that the observer described the phenomenon as instantaneous.

The letters *b* and *g* refer to the definition, the first indicating that it was bad, the second that it was good.

When there is no indication in this column the observer gives no information respecting the phenomenon.

Observer.	Place.	Reduced Time of Int. Contact.	Aperture.	Power.	Character of Phenomenon.
		h. m. s.			
Oppolzer,*	Vienna,	20 59 49·3	4	--	B. D. b.
Oppenheim,	Bonn,	51·8	--	--	b.
Rayet,	Paris,	57·0	5·5	222	V. C.
Le Verrier,†	Marseilles,	57·6	--	--	B. D. (Inst.) g.
Lynn,	Greenwich,	59·2	?	170	B. D.
P. J. Kaiser,	Leiden,	59·2	3	--	R. R. (?) b.
Wolff,	Bonn.,	21 0 1·3	--	--	b.
Liais,‡	Atalaia,	1·5	--	78	V. C. (g.)
Rosen,	Pulkowa,	2·7	5	59	
Stone,§	Greenwich,	4·0	12¼	245	B. D.
Lais,	Rome,	4·6	2	40	R. R.
Kartazzi,	Pulkowa,	4·7	2·9	145	
Wagner,	"	4·7	3·7	148	
Nyren,	"	4·7	7·4	57	
Fuss,	"	4·7	2·4	63	
Dunkin,‖	Greenwich,	4·7	3·7	300	B. D.
André,	Paris,	4·9	5	188	V. C.
Argelander,	Bonn,	5·3	--	--	b.
Oppolzer,	Vienna,	6·3	4	--	A. C.
Dr. Peters,	Altona,	7·3	4	111	b.
Villarceau,	Paris,	8·3	7·3	163	V. C.
Lebedeff,	Pulkowa,	8·7	2·2	70	
Dollen,	"	8·7	2·2	81	
Miroschnit schenko,	"	8·7	3·7	106	
Leskinen,	"	8·7	3·9	117	
Mancini,¶	Rome,	9·5	6	60	R. R.
H. J. Carpenter,	Greenwich,	9·6	?	70	R. R.
Kasavinoff,	Pulkowa,	10·7	1·1	36	
Kaiser,	Leiden,	10·8	7		B. D. b.
Criswick,**	Greenwich,	11·3	4?	170	B. D.
Wolf,	Paris,	11·4	8	200	V. C.
Secchi,	Rome,	11·5	7	200	R. R.
J. Carpenter,	Greenwich,	12·6	3·7	90	B. D. (Inst.)
Prof. Peters,	Altona,	13·3	3	145	b.
Weiss,	Vienna,	13·3	5	40	b.
Struve,	Pulkowa,	13·7	2·5	207	R. R.
Stephan,	Marseilles,	13·7	--	--	R. R. (Inst.)
Merino,	Madrid,	14·1	5	100	R. R. g.
Duner,	Lund,	14·2	9	320	V. C. g.
Stone,	Greenwich,	16·0	--	--	A. C.
Buckingham,	Walworth,	17·3	--	--	B. D.
Sokolov,	Pulkowa,	18·7	2·8	226	
Kam,	Leiden,	19·0	6	--	R. R. (?) b.

* "8ˢ vor dem Brechen des Lichtfadens schien Mercur gleichsam den Sonnenrand etwas hinauszuchieben so dass an der Stelle des Austrittes ein nach aussen convexer Lichtfaden den Mercur umschloss und dessen Verschwinden ich als den Moment der inneren Berührung notirte."

† Le Verrier used a seven-inch glass with a diaphragm over the objective.

‡ Three seconds before this time the contact was not completed, and three seconds later it was more than completed.

§ A fine ligament seen several seconds before the time recorded.

‖ The planet was suddenly noticed to assume a balloon or pear shape. The apparent contact about 10ˢ later.

¶ Observed by image in camera obscura, 22 c. m. in diameter.

** Instantaneous. Time perhaps 2ˢ or 3ˢ late.

TABLE—continued.

Observer.	Place.	Reduced Time of Int. Contact.	Aperture.	Power.	Character of Phenomenon.
		h. m. s.			
Ventosa,*	Madrid,	21·0	1·0	150	B. D. (Inst.)
Kaiser,	Leiden,	23·8	7	--	A. C. b.
Lindeman,	Pulkowa,	24·7	4·2	85	
Prince,	Uckfield,	28·5	--	--	A. C. (?)
Pohl,	Kahlenberg,	29·6	4	135	V. C.
Lynn,†	Greenwich,	31·1	?	170	A. C.
Penrose,	Wimbledon,	52·2	2·2	70	R. R.

In the preparation of this table, the original intention was to exclude all observations unaccompanied with any statement of the phenomena. An exception was afterward made in favor of the observations of Bonn and Pulkowa, so that in this respect there is a lack of homogeneousness in the table. The few observations of "apparent contact," have also been added, in the belief that they would not be devoid of interest.

The principal conclusion, to be drawn from the comparison here exhibited, is that there is no discoverable relation between the time of observation on the one hand, and the size of the telescope, the magnifying power, (so it exceed 50 or 60) or the character of the phenomenon on the other. We find the phenomenon of the apparently instantaneous formation of the "black drop" to range from $20^h\ 59^m\ 58^s$ (Le Verrier) to $21^h\ 0^m\ 21^s{\cdot}0$ (Ventosa). The times of "Vanishing Contact" on which Wolf and André lay so much stress, range from $20^h\ 59^m\ 57^s$ to $21^h\ 0^m\ 29^s$. If we reject Pohl's observation, the range will still be 17^s. I think if the observations of external contact were collected and compared in the same way, it would be found that their agreement was as good as in the case of internal contact. So far as we have data for judging, these differences would seem to be due to the accidental errors of observation. Their amount in arc may be inferred from the fact that 15^s of time correspond to a change of $1''$ in the relative position of Mercury and the sun.

I conceive, therefore, that we shall fail if we rely mainly on observations of internal contact. Still, there are two measures by which the reliableness of the determination of ingress and egress may be greatly increased. The first consists in having the observer occupy the entire time of partial ingress and egress in making very careful measures of the distance of cusps with such micrometer as may be best adapted for the purpose. The second consists in bringing the observers at opposite stations together, both before and after the transit, and causing them to make observations on artificial transits with the same instru-

* Contact seemed to be established simultaneously at several points.

† Doubtful. Limbs "certainly in contact by at least that time."

ments employed in observing the transit of Venus, in order to determine what correction should be applied to the observations of one to make them comparable with those of the other. It would be a comparatively simple operation to erect an artificial representation of the sun's disk at the distance of a few hundred yards, and to have an artificial planet moved over it by clockwork. The actual time of contact could be determined by electricity, and the relative positions of the planet and the disk by actual measurement. With this apparatus it would be easy to determine the personal errors to which each observer was liable, and these errors would approximately represent those of the observations of actual transit.

Still, it would be very unsafe to trust entirely to any determination of ingress or egress. Understanding the uncertainty of such determinations, the German astronomers have proposed to trust to measures with a heliometer, made while the planet is crossing the disk. The use of a sufficient number of heliometers would be both difficult and expensive, and I think we have an entirely satisfactory substitute in photography. Indeed, Mr. De la Rue has proposed to determine the moment of internal contact by photography. But the result would be subject to the same uncertainty which affects optical observations—the photograph which first shows contact will not be that taken when the thread of light between Venus and the sun's disk was first completed, but the first taken after it became thick enough to affect the plate, and this thickness is more variable and uncertain than the thickness necessary to affect the eye. We know very well that a haziness of the sky which very slightly diminishes the apparent brilliancy of the sun, will very materially cut off the actinic rays, and the photographic plate has not the power of adjustment which the eye has.

But, although we cannot determine contacts by photography, I conceive that we may thereby be able to measure the distance of the centers of Venus and the sun with great accuracy. Having a photograph of the sun with Venus on its disk, we can, with a suitable micrometer, fix the position of the center of each body with great precision. We can then measure the distance of the centers in inches with corresponding precision. All we then want is the value in arc of an inch on the photograph plate. This determination is not without difficulty. It will not do to trust the measured diameters of the images of the sun, because they are affected by irradiation, just as the optical image is. If the plates were nearly of the same size, and the ratio of the diameters of Venus and the sun the same in both plates, it would be safe to assume that they were equally affected by irradiation. But should any difference show itself, it would not be safe to assume that the light of the sun encroached equally upon the dark

ground of Venus and upon the sky, because it is so much fainter near the border.

If the photographic telescope were furnished with clock-work, it would be advisable to take several photographs of the Pleiades, both before and after the transit, to furnish an accurate standard of comparison free from the danger of systematic error. There is little doubt that if the telescopes and operators practice together, either before or after the transit, data may be obtained for a satisfactory solution of the problem in question.

To attain the object of the present paper, it is not necessary to enter into details respecting choice of stations and plans of observation. I have endeavored to show that no valuable result is to be expected from hastily-organized and hurriedly-equipped expeditions; that every step in planning the observations requires careful consideration, and that in all the preparatory arrangements we should make haste very slowly. I make this presentation with the hope that the Academy will take such action in the matter as may seem proper and desirable.

ART. XI.—*On the Geology of Eastern New England;* by Dr. T. STERRY HUNT, F.R.S. (From a letter to Prof. JAMES D. DANA.

WHEN, more than twenty years since, my attention was turned to the geology of New England, there was no evidence of the existence between the old gneisses of the Adirondacks and the coal measures, of any other stratified rocks than those of the Huronian series, and the New York system, from the Potsdam formation, upward. It is true that Emmons had, before that time, maintained the presence, in western Vermont and Massachusetts, of a system of fossiliferous sediments, lying unconformably beneath the Potsdam, but the evidence up to this time adduced with regard to these so-called Taconic rocks, has failed to show that they include any strata more ancient than the Potsdam, while most of them are certainly younger. The researches of Sir William Logan, up to 1848, had led him to refer to a period not older than the Lower Silurian the crystalline sediments of the Appalachian region of Canada, between Lake Champlain and Quebec. These form a chain of hills, the continuation of the Green Mountains, and were found by him to be followed immediately, to the southeast, by more or less calcareous and somewhat altered strata, associated with Upper Silurian fossils, and succeeded, across the strike, near the sources of the Connecticut River, by a series, several miles in breadth, of micaceous schists and quartzose strata, occasionally containing

chiastolite, garnet and hornblende. These two series of rocks, extending from the base of the Green Mountains to Canaan on the Connecticut, it was suggested by Sir William Logan, in his Report on the Geological Survey, 1847–1848, might be the altered representatives of the rocks of Gaspé, including the Lower Helderberg group, and the succeeding members of the New York system to the top of the Chemung. I then, as now, conceived that these micaceous and argillaceous schists, often holding garnets and chiastolite, were identical with those which make so conspicuous a figure in the White Mountains, and elsewhere in Eastern New England, and when, in 1849, I laid before the American Association at Cambridge, the results of the Geological Survey of Canada (this Jour., II, ix, 19), suggested that to the Gaspé series, as above defined, "may perhaps be referred, in part, the rocks of the White Mountains." Lesley, subsequently, in 1860 (Proc. Philad. Acad. Nat. Sciences, page 363), adduced many reasons for believing that the rocks of these mountains might be strata of Devonian age.* In the large geological map of Canada and the northern United States, lately published by Sir William Logan, no attempt is made to delineate the geology of New Hampshire, but the rocks in question, to the north of the United States boundary, are represented as Upper Silurian, with the exception of a belt of the Quebec group, which has been recognized in that region.

In fact, the schists and gneisses of the White Mountains are clearly distinct, lithologically, from the Laurentian, the Labradorian and the Huronian, as well as from the crystalline rocks of the Green Mountains, and from the fossiliferous Upper Silurian strata which lie at the southwestern base of the Canadian prolongation of the latter. Having thus exhausted the list of known sedimentary groups up to this horizon, it was evident that the crystalline strata of the White Mountains must be either (1) of Devonian age, or (2) something newer (which was highly improbable); or (3) must belong to a lower and hitherto unknown series. In the absence of any proof, at that time, of the existence of such a lower system, the first view, which referred these strata to the Devonian period, was the only one admissible.

* In this connection should be recalled the views put forth in 1846, by Messrs. H. D. and W. B. Rogers, in a paper on the Geological Age of the White Mountains, (this Journal, II, i, 411). They there, for the first time, pointed out that the great mass of these mountains consists of more or less altered sedimentary strata, which, upon the evidence of supposed organic remains, they referred, with some little doubt, to the Clinton division of the Upper Silurian. In 1847, however, they announced that the supposed fossils, on which this identification had been founded, were not really such, (this Journal, II, v, 116). Future explorers may, it is hoped, be more successful, and yet discover among the strata of the White Mountains evidences of organic life, probably of primordial Silurian age.

When, however, further investigation showed that the great and progressive thickening which takes place in the paleozoic formations from the west, eastward, is not confined to the augmentations of existing subdivisions, but includes the intercalation of new ones; when the few hundred feet of typical Potsdam sandstone in New York are represented in Vermont, Quebec and Newfoundland, by thousands of feet of strata lithologically very unlike the type; while the Quebec group, not less in volume, appears representing the beds of passage between the Calciferous and Chazy divisions of New York, we begin to conceive that conditions of sedimentation, very unlike anything hitherto suspected in the west, prevailed to the eastward. When, moreover, we find widely separated areas of Labradorian and Huronian rocks,—remaining fragments of great series,—resting upon the Laurentian, from Lake Huron to Newfoundland, we get evidences of a process of denudation in past ages, not less remarkable than the sedimentation.

My observations of last year have led me to a conclusion, which had previously been taking shape in my mind, that there exists above the Laurentian, a great series of crystalline schists, including mica-slates, staurolite and chiastolite-schists, with quartzose and hornblendic rocks, and some limestones, the whole associated with great masses of fine-grained gneisses, the so-called granites of many parts of New England. The first suggestions of this were given me by the observation of Dr. Bigsby, confirmed by specimens since received from the region, that there exists to the northwest of Lake Superior, an extended series of crystalline schists, unlike the Laurentian, and resembling those of the White Mountains. I have already called attention to this resemblance in a review of the progress of American Geology, in 1861 (this Jour., II, xxxi, 395). It was contrary to my notions of the geological history of the continent to suppose that rocks of Devonian age could, in that region, have assumed such lithological characters, and I was therefore led to compare these rocks with a great series of crystalline schists, abounding in mica-slates and micaceous limestones, which occupy considerable areas in the Laurentian region in Hastings county, to the north of Lake Ontario. The distribution of this series has been traced out by Mr. Vennor, who, in 1869, was able to show that, although much contorted, it rests unconformably upon the old Laurentian gneisses, while it is, at the same time, overlaid by the horizontal limestones of the Trenton group. This intermediate series, which attains a thickness of several thousand feet, is terminated by calcareo-micaceous schists, in which *Eozoon Canadense* has been found, both in Madoc and in Tudor. In these localities, as shown by Dawson and Carpenter (this Jour., II, xliv, 367), the calcareous

skeleton of the Eozoon, instead of being injected by serpentine or another silicate, is simply filled with impure calcareous and carbonaceous matter. The presence of this fossil serves to connect these rocks with the Laurentian system, with which they had provisionally been classed, although their lithological dissimilarity had long been noticed, and in 1866 Sir William Logan had remarked their resemblance to the mica-slate series found near the sources of the Connecticut River (Report Geol. Survey, 1866, p. 93).

Mr. Alex. Murray's report of his explorations in Newfoundland, published in 1866, throws much light on the history of the rocks immediately succeeding the Laurentian in that region. He found in the great northern peninsula, about the Cloud Mountains and Canada Bay, not less than 5400 feet of strata, referred by him to the Potsdam group. Of these the lower 2500 feet consist of bluish-gray slates, holding near the summit, beds which become conglomerate from the presence of quartz pebbles, and are followed by a mass of purplish amygdaloidal diorite, holding epidote and jaspery red iron ore. Then follow 2000 feet of argillaceous and somewhat micaceous slates, with beds of quartzite and of limestone, generally impure. These contain, besides numerous fucoidal markings, the remains of a Lingula, and of *Olenellus Vermontanus*, a fossil characteristic of the Potsdam group. To this second division succeeds a third, consisting of about 900 feet additional of limestones and slates. Somewhat farther southward, at Great and Little Coney Arms, the lower half of the above series is not observed, but a succession of strata, supposed to represent the upper portion of the Potsdam, is more particularly described. It consists, at the base, of 300 feet of pale bluish-gray mica-slates, with iron stains, "softer, more finely laminated, and more uniform both in color and in texture" than some micaceous strata described by Mr. Murray as occurring in the Laurentian in that region. To these succeeded 430 feet of similar soft bluish-gray mica-slates, holding numerous thin seams of dark colored limestone, and followed by 1000 feet of impure limestones and slates, often micaceous and calcareous, among which are a few beds of white compact marble. No indications of fossils, save fucoidal markings, were met with in this section. At Coney-Arm Head there is seen a series of "whitish granitoid, very quartzose mica-slates," which appear to have a thickness of from 1500 to 2000 feet. The same rock is found in White Bay, where it overlies what is supposed to be Laurentian gneiss. The relations of these whitish granitic mica-slates are still obscure, but Mr. Murray was inclined to regard them as occupying a position beneath the Potsdam group. The latter, in Canada Bay, is immediately followed by the unaltered fossiliferous limestones and shales of

the Quebec group. From these investigations of Mr. Murray we learn that between the Laurentian and the Quebec group, there exists a series of several thousand feet of strata, including soft bluish-grey mica-slates and micaceous limestones, belonging to the Potsdam group; besides a great mass of whitish granitoid mica-slates, whose relation to the Potsdam is still uncertain. To the whole of these we may perhaps give the provisional name of the Terranovan series, in allusion to the name Newfoundland.

Imperfect gneisses and micaceous schists are found in several parts of the province of New Brunswick, associated with what has been described as a great granitic belt. These rocks have been examined by Prof. Hind, and by Mr. Robb, on the St. John and Mirimichi rivers; and the former of these observers some years since pointed out the indigenous character of the so-called granites. In the summer of 1869 I had an opportunity of examining, with Prof. L. W. Bailey, the region about St. Stephen, on the river St. Croix, where he had already observed a series of ferruginous quartzites and imperfect gneisses, accompanied by soft bluish mica-slates sometimes holding chiastolite, staurolite, and garnet. These highly crystalline schists are not more than five miles removed from unaltered shales of the Gaspé series, containing fossils of Upper Silurian or Lower Devonian types, and rest unconformably upon older granitoid rocks, which Prof. Bailey regards as probably Laurentian. We subsequently examined the crystalline schists of the St. John, which are apparently identical with those of the St. Croix, and these also overlie, unconformably, an older granitoid gneiss.

More recently Prof. Hind has pointed out that some of the so-called granites of Nova Scotia are ancient gneisses, probably of Laurentian age, and have shown that between these and the gold-bearing slates of that province, there is found, near Windsor, and near Sherbrooke, a series of beds of no great thickness, consisting of imperfect gneisses, quartzites and micaceous schists, which rest unconformably on the Laurentian, and are sometimes wanting altogether. These include mica-schists with chiastolite and garnet, and appear identical with those already observed by Dr. Dawson in other parts of Nova Scotia, which I had already recognized as the same with those of the White Mountains, and those of the St. Croix, just noticed. Prof. Hind, in a late paper, has called these, from their position in Nova Scotia, Huronian; but the Cambrian or Huronian rocks recognized by Messrs. Matthew and Bailey in New Brunswick, where they are widely spread along the north side of the Bay of Fundy, consist of massive diorites and quartzose feldspar-porphyries, with occasional sandstones and conglomerates, and are very unlike the gneissic and micaceous rocks in question,

which I believe to belong, like those of the St. Croix and the St. John rivers, to the great Terranovan series. The micaceous and hornblendic schists, with interstratified fine grained whitish gneisses (locally known as granites) which I have seen in Hallowell, Augusta, Brunswick and Westbrook, in Maine, appear to belong to the same series; which will also probably include much of the gneiss and mica-schist of Eastern New England. If this upper series is to be identified with the crystalline schists which, in Hastings County, Ontario, overlie, unconformably, the Laurentian, and yet contain *Eozoon Canadense*, the presence of this fossil can no longer serve to identify the Laurentian system. To this lower horizon however, I have referred a belt of gneissic rocks in Eastern Massachusetts, which are lithologically unlike the present series, and identical with the Laurentian of New York and Canada. To the upper series appear to belong the great endogenous granitic veins so well known to mineralogists as containing beryl, tourmaline and other fine crystallized minerals.

The fine-grained, white granitoid gneisses, often present an apparently bedded structure, which enables them to be removed in large plates or layers, lying at no great angle, and apparently conformable to the present surface of the country. This structure, which I conceive to have been superinduced by superficial changes of temperature, is often quite independent of the bedding, as may be seen in the quarries near Augusta in Maine, and in the cuttings on the Grand-Trunk Railway near Berlin Falls, New Hampshire. It is also observed in exotic or intrusive granites, like those of Biddeford, Maine. This is, in fact, the concentric lamination of granite, long since observed by Von Buch, and, I believe, correctly explained by Prof. N. S. Shaler to be due to movements of contraction and expansion in the mass, caused by variation of temperature during the changes of the seasons. He has not however observed this structure at greater depths than from three to five feet, while in some rocks I have found it penetrating probably twenty feet. (See Shaler's paper, read before the Boston Nat. History Society, Feb. 3, 1869, and published in the Proceedings of the Society, vol. xii, page 289).

While however I admit the existence in the Dominion of Canada and in Eastern New England, of a great series of crystalline schists, distinct from the Laurentian, and apparently the same with those found by Mr. Murray between the Laurentian and the Quebec group in Newfoundland, it is not less certain that we have in these regions rocks of Upper Silurian and Lower Devonian age, holding characteristic fossils. These strata in Maine and New Brunswick are generally but little altered. In the Connecticut valley at Bernardston, Massachu-

setts, near Lake Memphremagog in Vermont, and further northward in the province of Quebec, fossils of this horizon are found in rocks which, in some localities, are more or less altered and crystalline. I believe however that much of the calcareous mica-slate of Eastern Vermont will be found to belong to the Terranovan series. The extent of these newer rocks, and the limits between them and the more ancient schists, of the ruins of which they are probably in part composed, remain problems for farther investigation. For the solution of these, Prof. C. H. Hitchcock, by his labors in Vermont, is already well prepared, and it cannot be doubted that he, with his able assistants, will in the Survey of New Hampshire, now in progress, throw much light on New England geology. It is worthy of remark, that strata holding fossils of Lower Helderberg age, or thereabouts, are not confined to the shores of Maine and New Brunswick, and the valleys of the Connecticut and St. John rivers, but are found beyond the Green Mountains, in the valley of the St. Lawrence, near Montreal; where, on the island of St. Helen, they rest unconformably on the Utica slate, and at Belœil Mountain, near by, on intrusive diorites, which there break through the shales of the Hudson River group.

The relations of this Terranovan series to the porphyries and diorite rocks which, in New Brunswick, have been called Cambrian and Huronian by Mr. Matthew (first distinguished by him as the Coldbrook group), yet remains to be determined. These rocks are found near to the city of St. John resting directly on what has been regarded as Laurentian, and are overlaid by the uncrystalline schists which contain the primordial fauna now so well known by the descriptions of Prof. Hartt. Rocks which I regard as identical with this same Coldbrook or Cambrian group, are found along the coast of New Brunswick, and constitute the diorites and porphyries of Eastport, Maine. They appear moreover to be the same with those met with near Newburyport, and at Salem, Lynn, and Marblehead, Massachusetts. Farther researches about Passamaquoddy Bay, where the mica-slates are found not far removed from these porphyries, will probably enable us to determine their relations to each other.

It will be remembered that Gümbel has found, in Bavaria, beneath the oldest fossiliferous clay-slates, a mica-schist (and hornblende-schist) series, reposing upon the Hercynian gneiss, which contains crystalline limestones, with graphite, serpentine and *Eozoon Canadense*, and which he has identified with the Laurentian of North America. He distinguishes beneath this a great mass of red gneiss, apparently without limestones, to which he has given the name of the Bojian gneiss. It will however be remembered, that in his studies of the Laurentian system on the

Ottawa, Sir William Logan has shown that this immense series (his Lower Laurentian), some 20,000 feet in thickness, includes four great masses of gneiss and quartzite, divided by three limestone formations, and that it is in the uppermost of these, which is, in some parts, 1500 feet thick, that the *Eozoon Canadense* has been found. Some of the lower gneisses of this vast system may very well represent the Bojian of Gümbel, who has not recognized in Bavaria either the Labradorian (Upper Laurentian) or Huronian series. (See Gümbel on the Laurentian of Bavaria, translated and published in the Canadian Naturalist for December, 1866). Comparative studies of this kind should not be neglected in the investigation of our American rocks.

Montreal, May 10, 1870.

ART. XII.—*Mineralogical Contributions;* by CHARLES UPHAM SHEPARD, Sr.

1. *A new variety (species?) of Columbite.*

THE Columbites of New England deserve a closer examination than they have yet received. Those of New Hampshire, Massachusetts and Connecticut may embrace at least two, perhaps three, mineralogical species. A new locality, recently opened at Haddam (Conn.), has certainly disclosed a variety so different from the productions of the old one on the farm of Mr. Brainard, as to have led to doubts among collectors whether it fairly belongs to the Columbite group.

For my first knowledge of the new variety, I am indebted to several of my pupils who visited Haddam during the past year; and particularly to Mr. Charles H. Ames of the Senior class in Amherst College, who has furnished me with several dozen specimens, mostly isolated crystals, from one ounce in weight, downward, to that of a few grams. They all occur at a single repository, contiguous to the center of the village, and directly in rear of the house of Mr. Nathaniel Cook, the well known collector and dealer in Haddam minerals. The locality was opened by explorers for porcelain stone; but the feldspar proving too ferruginous, being of a somewhat flesh-red color, has led to an abandonment of the enterprise. Although this discovery is a recent one, it is not unlikely that specimens from the same spot had previously found their way into cabinets; and may thus have led to some of the discrepancies existing in the various descriptions of the Connecticut columbites.

The general aspect of the crystals is rather peculiar. Instead of flattened prisms, they are nearly all square and often, through imperfection, decidedly rhombic. With the common

goniometer, many of them give angles nearly as oblique as pyroxene. Nevertheless, when more carefully studied by means of the reflective goniometer, accompanied by observations on the faces replacing the lateral prismatic edges, no essential distinction of form can be made out between these crystals and those of columbite or of tantalite. They are all strictly isomorphous. An unimportant, though striking peculiarity is observed in these crystals, proceeding from the unusual development of the octahedral faces, whereby they possess a four-sided pyramidal summit at one, or each extremity. Very rarely are traces of the terminal plane visible; and when seen, the eye or the common goniometer would always report it as slightly wanting in verticality. Its smoothness is insufficient for measuring its position with the reflective goniometer.

The faces of the crystals are wanting in that high luster so frequent in those from other localities. In luster, color and even in general shape, they resemble on first inspection, the average specimens of Elba yenites nearly as much as they do the ordinary Haddam columbite,—the chief exception being, that the narrow faces about the prismatic edges are bright.

No pavonine tints are seen in the present variety. Their absence is so marked, not only on natural planes but also on fractured surfaces, as to afford a ready and almost sure criterion for distinguishing specimens of this locality from those obtained at the old one of Brainards, where every crystal and almost every fragment displays the blue or yellowish tarnish.

The color of the fracture is perfectly black approaching pitchy. It is more firm, and less prone to crumble than the ordinary columbite. The fracture is occasionally small conchoidal. No distinction in hardness can be made out. It is nevertheless more easily ground to a fine powder than the usual mineral; and, when perfectly powdered, presents an almost black color, while columbite is only brownish black. This distinction is so marked, as to be recognized by candle light.

But the most remarkable distinction resides in its lower specific gravity, which may be said to be strictly uniform. Eight examples (varying in weight from 2 to 16 grams) gave 5·31 as the average. Three of these gave respectively, 5·32, 5·34 and 5·35; while four specimens of columbite from the Brainard locality gave 6·02, 6·03, 6·10 and 6·19,—the average being 6·085.

Its powder, strongly heated in an open platinum crucible, lost only ·02 p. c. in weight, but changed in color, by several shades, to a brownish black.

Alone before the blow-pipe, small fragments had their edges decidedly rounded by fusion. Treated in powder in small

quantity, in the outer flame with borax on platinum wire, a clear, pale yellow globule (while hot) with faint tinge of brown, was obtained, which on cooling became almost colorless. In the interior flame, no change took place, except that of greater paleness in the globules. The addition of nitre produced the reaction for manganese.

The columbite powder from Brainards' gave similar results, with the difference, of deeper tints to the glass.

With microcosmic salt, the behavior was similar, excepting when the mineral was added in excess; a brownish red globule was then afforded.

Heated with carbonate of soda on charcoal, it yielded, in common with the columbite, minute granules of tin.

It is feebly, if at all, attacked by hydrochloric acid; but is perfectly decomposed by heated sulphuric acid. The action is evinced almost immediately,—the mixture becoming first yellowish, then greenish yellow, and finally deep yellow, which by the affusion of water turns white. The Haddam columbite, on the other hand, is only imperfectly decomposed by the same treatment. Both minerals, however, are speedily decomposed by fusion with bisulphate of potash,* but with a marked difference in the aspect of the fused mass; the new variety yields a honey yellow, transparent glass, while the columbite gives one which is grayish white and opaque.

A portion of the white insoluble residue (after decomposition of mineral with sulphuric acid) on being treated on tinfoil with hydrochloric acid, instantly produced a deep Prussian blue liquid and precipitate. On dilution with water, the color grew paler; and, in half an hour, even the sediment had nearly changed to a grayish blue.

Another portion of the above moist powder was treated with dilute sulphuric acid and a strip of zinc. The metal was instantly coated by a dark blue precipitate, which after two hours also changed to ash-gray.

The original sulphuric acid solution, slightly diluted with water, after separation from the insoluble white precipitate, was boiled; whereupon a considerable white precipitate of the metallic acid, (or acids,) was thrown down. This on being heated was of a faint yellow color, and nearly white after cooling. It tinged borax pale citron yellow while hot, but left it colorless on cooling; and gave similar results with microcosmic salt.

A portion of the metallic acids was fused with caustic soda in a silver crucible. The mass obtained was dissolved in water; and through the solution a stream of carbonic acid was

* This easy decomposition, which is the most striking in the new variety, points also to the comparative absence of tantalic acid.

conducted to complete saturation, producing no precipitate, whereby the absence of tantalic acid was inferred.*

Analysis, the decomposition being effected by fusion with potash, gave the composition as follows:

Metallic acids	78·30
Protoxyd of Iron	13·86
Protoxyd of Manganese	7·72
Tin only in traces.	
	100·28

In two processes for the metallic acids, one by the decomposition of the mineral by sulphuric acid, the other by potash, an exact agreement in specific gravity was found, viz.: 4·06; from which, perhaps, it may be just also to infer the absence of tantalic acid, whose density when pure, though varying somewhat according to its mode of preparation, averages fully one-third higher than the present result.

Hermann distinguishes three varieties of columbite, viz.: (1) with gravity above 5·9, (2) with gravity 5·5 to 5·9, and (3) with gravity 5·4; but it is very doubtful whether an example of the mineral under notice has ever fallen into his hands, as this locality is of recent discovery. In conclusion, it may be added that minute coatings of uranochre were observed in patches and specks upon a few of the crystals; though no evidence of the presence of uranium was afforded in the mineral itself.

2. *Unknown mineral (microlite?) in Haddam columbite.*

This mineral I suspect to be new. It has never fallen under my notice until found among the productions of a recent blast-

* The following reactions were obtained in testing a solution of the above compound of soda with the metallic acid; with protonitrate of mercury, a curdy, dense precipitate of a pale yellowish white, which after fifteen minutes assumed a green tint; with corrosive sublimate, no precipitate, but by slight evaporation, it thickened, and by drying, still farther, a red-brown oxychlorid of mercury was deposited; sulphate of soda gave no precipitate, but sulphate of potash a feeble one after a little standing; ferrocyanid of potassium, no change at first, but a pale yellowish one afterwards; phosphoric acid rendered the solution feebly opaline at the end of two hours; cyanid of potassium produced immediately a thick, white precipitate; infusion of nut-galls no effect until a drop of hydrochloric acid was added, when a thick, orange-red precipitate appeared; nitric acid gave an abundant precipitate that did not dissolve by heating; hydrochloric acidproduced a voluminous precipitate; a portion of the solution was evaporated to $\frac{1}{4}$th its bulk, whereby a portion of the salt was thrown down, which was redissolved, with exception of a faint opalinity in the liquid; nitrate of silver added to this last solution produced an abundant pale yellow precipitate that was redissolved by boiling, a few drops of ammonia changed its color to brown, a larger quantity dissolved it entirely, and by drying in porcelain, it became first brown, than black. The foregoing reactions are those heretofore supposed to belong to the soda salts of niobium, or columbium; but it is probable that they may be shared to some extent by those of the allied metal, ilmenium.

ing of the columbite at the Brainard locality. It occurs very sparingly (for the most part requiring the use of the microscope for its detection), occupying the sides of open crevices in large crystals of columbite, and also between the surfaces of these crystals and feldspar. At first, I was led to suspect them to be cassiterite; but blow-pipe examination with soda on charcoal only afforded minute traces of tin. The high adamantine lustre next suggested zircon and tungsten. The color was intermediate between that of wöhlerite and monazite; but a closer examination showed a wide divergence in other properties from either of these species.

Apparently equilateral triangular planes were detected; and finally portions of both pyramids, which leave little doubt that the form is that of the regular octahedron, especially as two of the solid angles were each similarly replaced by four planes, resting on the octahedral faces. To verify this conjecture of a cubic primary, will require the breaking up of the only specimens I possess of the crystals, which for the present I defer in the hope of obtaining other examples. The hardness is 5· to 5·5. Brittle, fracture conchoidal, semi-transparent to translucent. Luster of fracture, resinous; that of surface of crystals, adamantine. Streak yellowish white.

Before the blow-pipe, infusible. After long heating, it loses its brown tint, and while still hot, assumes a feeble citron yellow color, which becomes paler on cooling, when it is seen to have lost much of its luster, and is also less translucent. With microcosmic salt in the outer flame, it slowly dissolves into a perfectly colorless, transparent glass; but in the interior flame, it becomes, while warm, slightly milky with a tinge of blue. An intermittent flame produced the same transparency and tinge of blue. Long continued heating produced no trace of brown, violet, amethyst or yellow. The addition of tin gave a bluish white semi-opaque pearl.

It approaches most nearly to the microlite, but differs from it in the absence of all cleavages as well as in luster and in its behavior before the blow-pipe. Its mode of occurrence and properties lead to the suspicion of its being chiefly, if not wholly, composed of one of the metallic acids of the columbium group. The microlite, though occurring within a few yards of the columbite at Chesterfield and in the same ledge of granite, was never seen associated with this species, but invariably presents itself either in the albite or crystals of tourmaline, while the present mineral at Haddam occurs solely in, or upon, the columbite.

3. *New locality of Bismuthine and Bismutite in Haddam.*

About thirty years since I pointed out the existence of both these species and a third bismuthic mineral, the bismucone, as

existing together in small quantity, associated with chrysoberyl, yellow beryl, columbite, garnet and a zircon mineral which I afterwards called calyptolite. Mr. Ames has lately procured for me large crystalline masses of bismuthine, more or less coated by bismutite, from a quarryman who had discovered them on the iolite hill, situated about one mile to the southwest of the meeting-house. They had apparently formed a narrow seam in the common orthoclase granite, which had been opened with a view to working as a porcelain-feldspar quarry. One of the specimens weighs about half a pound; and constitutes a deeply striated or channeled crystal. The mineral was supposed to be molybdenite. Examined before the blow-pipe, it affords no indications either of tellurium or selenium.

4. *On the metallic acid in Microlite.*

It having been suggested by Prof. Brush, from blow-pipe experiments* and the higher percentage of the metallic acid in this mineral, that it is the tantalic instead of the columbic, I made the following experiments upon two small crystals whose weight together was 0·350 gram. It was decomposed by fusion with bisulphate of potash, cold water dissolved the mass, which afterwards deposited a white granular precipitate, the clear supernatant liquid not showing even an opaline tendency on ebulition. The white precipitate after ignition gave 72·80 p. c. for the metallic acid. It evinced scarcely the slightest yellow tint when hot; and when cold, was perfectly white.

A portion of it gave with microcosmic salt, a perfectly clear glass, which showed only a faint yellowish tinge while hot. The addition of sulphate of iron produced a blood red tint when heated in the inner flame; on cooling, it changed to dirty yellow. With borax, it dissolves almost without color, being only a faint greenish yellow; but the globule inclines to opacity. No tinge of violet or pink is visible.

Fused with soda, the metallic acid afforded a bluish gray mass. The excess of alkali being removed by cold water, the addition of more water took up the residuum. A portion of this solution on being acidulated with hydrochloric acid, and afterwards treated with solution of nutgalls, gave a very yellow orange precipitate, quite different from that produced by columbic acid, which when contrasted therewith was seen to have a deep reddish tint† as compared with columbic acid.

The chief portion of the sodic solution was then supersaturated with carbonic acid, and gave rise to a decided

* Dana's Mineralogy, p. 514.

† There was a want of correspondence also in the reactions of the metallic acid and soda compound, with nitrate of silver, nitrate of mercury, corrosive sublimate and ferrocyanid of potassium.

precipitate. From the foregoing results, it would seem little doubt remains that microlite is a tantalate rather than a columbate of lime.

5. *Redondite.*

I have examined still farther the hydrous phosphate of alumina and iron described by me in a previous number of this Journal (vol. xlvii, p. 338), as occurring so abundantly in the island of Redonda, W. I., and am now of the opinion that it constitutes a species distinct from barrandite, from which it differs in several particulars, and essentially in specific gravity. Barrandite has gr.=2·576. Redondite gives gr.=2·019, which may be presumed to be a little too high, as the three examples used in the determination were found on analysis to give 8·8 p. c. of silica.

The phosphoric acid was determined on the same specimens by the molybdate of ammonia process; and amounted to 40·192 p. c. But as the silica (8·8 p. c.) was obviously accidental, the proportion of phosphoric acid in the pure mineral will stand at 44·07 p. c. The water is 24·73 p. c.

6. *Phosphoric acid in the Diaspore of Chester, Mass.*

Heermann having found phosphoric acid in the diaspores of several localities, I thought it worth while to analyze a good crystal of this mineral from Chester. It was semi-transparent and of a rich hair-brown color with a faint tinge of violet. Sp. gr.=3·343. The phosphoric acid was determined by the molybdate of ammonia. I obtained

Water,	15·80
Phosphoric acid,	0·32
Protoxyds iron, with traces of manganese	0·38
Alumina not determined; but by difference	83·50
	100·00

7. *The Pelham Vermiculite?*

It occurred to me as possibly interesting to make some chemical examination of this rather curious exfoliating mica, described by Mr. Adams in a late number of this journal (vol. xlix, p. 271). It is very abundant, and to the eye apparently as homogeneous as other softened micas.

It loses on an average 7 p. c. of water by ignition. By digestion in hot aqua regia, half its weight comes into solution, leaving behind 50 p. c. of minute, colorless scales, closely resembling margaric acid. They are so remarkable for their uniformity of size, freedom from color, and pearly luster, as scarcely to suggest their inorganic composition. Under the microscope, however, they very nearly give the distinct lateral

cleavage lines of mica; nor have they the perfect elasticity of a true mica. They show no tendency to farther exfoliation when heated; before the blow-pipe they melt with difficulty on the edges into a colorless glass. Prof. DesCloizeaux was kind enough to examine some of these scales optically; and found them to be uniaxial, from whence it is probable that they belong to the species biotite.

The solution in aqua regia afforded the following result in reference to 100 parts of the dissolved material.

Alumina	14·00
Magnesia	13·68
Peroxyd of iron	32·00
Silica	24·00

No search was made for alkalies; but from the result obtained it is apparent that the magnesia in part explains the use made by the farmers of this material for a fertilizer, as described by Mr. Adams.

The point, however, which most interested me was the discovery thus accidentally made of the cause of the worm-like exfoliation in the mineral, viz: from the coating of the mica scales with a hydrated, argillaceous mixture, which probably owes its origin to the decomposition of some other species of the micaceous or chloritic family. In subjecting a fragment of the vermiculite of Millbury, Mass., to a similar action of aqua regia, the result was an abundance of brilliant green scales, probably belonging to the species ripidolite. It may therefore be suggested, that many earthy species of minerals (silicates) will under analogous treatment be found to be less homogeneous than has been supposed.

Amherst, May 7, 1870.

ART. XIII.—*Notice of a new Species of Gavial from the Eocene of New Jersey*; by Professor O. C. MARSH, of Yale College.

THE Museum of Yale College has recently received some interesting reptilian remains from the Eocene greensand of New Jersey, which indicate a new species of Gavial, considerably smaller than any Crocodilian heretofore discovered. These specimens, which were found together, and are evidently parts of the same skeleton, consist of various fragments of the skull, and ten vertebræ. The coössification of the neural arches of the vertebræ, and the almost entire obliteration in some of them of the sutures, would imply that the individual, although diminutive, was nearly or quite mature.

The few portions of the skull preserved are sufficiently characteristic to show that the animal had an elongated muzzle, and that the upper and posterior parts of the head were of the gavial type. The form of the parietal bone indicates, moreover, that the temporal apertures were large, and near together, with their adjoining sides nearly vertical. The quadrates are elongate, unusually straight on the inner side, and their condylar surfaces deeply and obliquely notched. The pneumatic foramen, on the upper surface of the quadrate near the inner edge, is very large, and characteristic. The teeth of this specimen were unfortunately not secured.

The vertebræ are well preserved, and present marked characters. The articular cup is transversely oval in the cervicals and anterior dorsals, and has its upper margin depressed in the posterior dorsals. The hypapophyses are simple and elongate. The neural canal of the cervical and anterior dorsal series is transverse, and sub-rectangular in outline, and the floor unusually broad and flat. In the posterior dorsals the canal, although still transverse, becomes less rectangular, with the broader portion above.

The principal dimensions of the tenth, or first dorsal, vertebra are as follows:—

Length of centrum,	15·	lines.
Transverse diameter of cup,	8·75	"
Vertical diameter of cup,	7·	"
Width of neural canal, in front,	6·	"
Height of neural canal, in front,	3·50	"

The species here described may perhaps prove to be generically identical with the one named by the writer *Thecachampsa Squankensis*, which is the only Crocodilian hitherto found in the Eocene of New Jersey. The genus *Thecachampsa*, however, as proposed by Prof. Cope, cannot yet be regarded as a valid one, since the concentric structure of the dentine, on which it was founded,* is not a character of generic importance; for it occurs in various other Crocodilians, and also in some of the Cetacea. The present remains, therefore, may be placed provisionally in the genus *Gavialis*, and the species be called *Gavialis minor*. It may readily be distinguished from *Thecachampsa Squankensis*, aside from its greatly inferior size, by the posterior dorsal vertebræ, which have the cup and neural canal transverse, instead of vertical, and especially by having the bodies of these vertebræ proportionally more elongate, and without the remarkable constriction, which is one of the most striking characters of the latter species.

* Proceedings Philadelphia Acad. Nat. Sciences, 1867, p. 143.

The present specimens indicate that the animal to which they belonged was quite slender, and about six feet in length. They were found by Hugh Hurley, Esq., in the Eocene greensand of Shark River, Monmouth county, New Jersey, and by him presented to the Museum of Yale College.

New Haven, Conn., June 10th, 1870.

ART. XIV.—*On Hydrogenium-amalgam;* by O. LOEW, of the College of the City of New York.*

WHEN zinc-amalgam is shaken with water, a slow decomposition of the latter takes place, recognized by the formation of flocculi of hydrated oxyd of zinc, and the evolution of small bubbles of hydrogen on allowing the mixture to stand for a time. This decomposition of water by zinc is intensified when a small quantity of bichlorid of platinum is present; a spongy body then being formed on the surface of the zinc-amalgam. This body I have found to be an alloy of hydrogenium and mercury.

In order to obtain the hydrogenium-amalgam on a larger scale, zinc-amalgam containing a few per cents of zinc† is shaken thoroughly with about an equal volume of a solution of bichlorid of platinum containing about 10 per cent of the chlorid, care being taken to keep the mixture cool. The zinc-amalgam swells up considerably, precisely as in the ammonium-amalgam experiment, and continues to evolve hydrogen till the decomposition of the amalgam is complete. I have found that the volume of the hydrogen thus developed was, in several experiments from 100 to 150 times that of the mercury employed. This hydrogen possesses a faint but peculiar odor.

When this amalgam of hydrogenium (prepared with about 5 per cent of zinc) is pressed directly after its preparation, between sheets of filtering paper and then spread out in a layer (not too thin) to the air, the temperature soon rises considerably, and vapors of water are formed, which may be condensed in a glass receiver. The finely divided platinum present is obviously the cause of this rapid oxydation of the hydrogenium. In this action of platinic chlorid upon zinc-amalgam oxychlorid of zinc is at the same time formed; and though this may be removed by means of hydrochloric acid, yet by this treatment a part of the hydrogenium-amalgam is destroyed. If, after this, it be

* Read before the Lyceum of Natural History, Chemical Section, Apr. 11.

† A zinc-amalgam containing more than 5 per cent of zinc is a solid, and in this state, is not well fitted for use, though it would doubtless produce more hydrogenium in the amalgam. A zinc-amalgam of 5 per cent is readily liquified at a moderate heat and should then be immediately used.

washed with water, it undergoes a very slow decomposition, the volume increases and bubbles of hydrogen escape through the water above. An addition of zinc-amalgam or sodium-amalgam greatly accelerates the decomposition of the hydrogenium-amalgam.

Platinum after perfect amalgamation does not act as energetically as in its nascent state; i. e., when precipitated on the amalgam. When platinum-amalgam is mixed with zinc-amalgam the decomposition of the water by zinc is extremely slow and the hydrogenium-amalgam does not appear for some time. Under certain conditions moreover, the hydrogenium-amalgam is formed without the aid of platinic chlorid. I had at one time about twenty pounds of mercury containing zinc, which was left standing in a bottle with water for three weeks; the hydrogenium-amalgam formed on the surface of the mercury, gradually decomposing above and being renewed from below.

Graham compares hydrogenium to the active modification of oxygen, and no doubt there is much analogy between them. We may distinguish three modifications of hydrogen; namely, (1) common *nascent* hydrogen formed by the action of sodium-amalgam on water, or by that of zinc and hydrochloric acid; it has a strong reducing power but cannot form hydrogenium-amalgam; (2) common gaseous hydrogen which has at common temperatures only a very weak reducing power; and (3) hydrogenium. There are reasons for believing that these differences may be expressed by the following formulæ:

[O]	[OO]	[OO]O
Antozone	Common oxygen	Ozone
α oxygen.	β oxygen.	γ oxygen.
[H]	[HH]	[HH]H
Common nascent or	Common gaseous or	Hydrogenium
α hydrogen.	β hydrogen.	γ hydrogen.

ART. XV.—*The Brachiopoda, a Division of Annelida;** by EDWARD S. MORSE.

AT a meeting of the Boston Society of Natural History, June 1st, 1870, Mr. Edward S. Morse made a verbal communication on the position of the Brachiopoda in the animal kingdom. He referred to the branch of Mollusca as it was understood forty years ago, when misled by external characters, many worms, like *Serpula* and *Spirorbis*, and a group of Crustaceans, the Cirripedia, were included with mollusks, and that from a proper recognition of their characters these diverse forms had

* Abstract from Proceedings Boston Society Natural History.

been eliminated from time to time, and referred to their proper branches. After long and careful study Mr. Morse was prepared to state that the Brachiopods were true Articulates, and not Mollusks, and that their proper place was among the worms, forming a group near the tubicolous Annelids.

He stated that for the past year he had been deeply engaged in the study of the Brachiopoda, and more particularly their early stages. Beside material from the coast of New England, he had had, through the kindness of Prof. A. E. Verrill, a large lot of *Discina* from Callao, Peru, belonging to the Yale College Museum. From these he had studied their early s ages, but as he had in preparation a memoir upon the subject, he would now confine himself to the considerations that follow.

He first spoke of the structure and composition of the Brachiopod shell, and pointed out the relations between the cœcal prolongations of the mantle in *Terebratula* and a similar structure in the test of Crustacea. He had also noticed a marked resemblance between the polygonal cells in the shell of a young *Discina*, and a similar feature in certain lower Crustaceans. The scale-like structure of the test of *Idotaea*, resembled the scale-like structure of *Lingula*. The skin of *Nereis* had similar punctures or dots, as seen in *Terebratula* and also in the peduncle of *Lingula*. He had submitted the shell of *Discina* to chemical tests and believed it to be chitinous. Gratiolet had already given the chemical analysis of *Lingula anatina* and found forty two per cent of phosphate of lime, and only six per cent of carbonate of lime. The position of the valves of all Brachiopoda were dorsal and ventral, and this was a strong articulate character to be compared to the dorsal and ventral plates of the Articulates. The horny setæ that fringe the mantle of Brachiopods was a feature entirely absent in the Mollusca, and peculiar to the worms.

The bristles of worms differ from those of other articulate animals in having sheaths containing muscular fiber, while in other Articulates, the hairs were simply tubular prolongations of the epidermal layer. In Brachiopods the setæ or bristles were secreted by follicles imbedded, or surrounded by muscular fibers, and were moved freely by the animal. In the structure of the setæ he found an identity with that of the worms. He then called attention to the resemblance between the lophophore of the Brachiopods and a similar structure in the tubicolous worms. In *Sabella* the cephalic collar was split laterally, and a portion of it reflected. Let this collar be developed so as to cover the fringed arms, and a representation of the mantle of Brachiopoda would be attained. The thin and muscular visceral walls suggest similar parts in the worms. The circulating system he had not sufficiently studied, though Dr. Gratiolet had stated

that in this respect there was a strong resemblance to the Crustacea.

In regard to the respiratory system, Burmeister had shown that there was a resemblance between the soft folds or lamellæ, developed on the internal surface of the mantle of *Balanidæ*, and similar features in *Lingula;* though the existence of these folds in *Lingula* had been questioned, he would presently show that Vogt was right in his observations. In regard to the reproductive system, he called attention to the fact that in one group of Cirripeds the ovaries were lodged in the upper surface of the peduncle, while in another group the same parts were lodged in the mantle. A similar condition existed in the Brachiopods where in one group the mantle holds the ovaries, while in another group they are found in the visceral cavity.

Through Polyzoa also he showed that, in their winter eggs or statoblasts, a relation was seen to the ephippia of *Daphniæ*, and the winter eggs of Rotifers.

Of great importance also, and upon which he laid particular weight, were the peculiar oviducts with their trumpet-shaped openings so unlike the oviducts of mollusks, and as he believed, bearing the closest affinity to the oviducts in many of the worms, namely: a pair of tubes, and in one case two pairs, having their inner apertures with flaring mouths, suspended in the visceral cavity, thus opening a direct communication between the visceral fluids and the surrounding media. He then called attention to what little information we had regarding the embryology of the Brachiopods. Lacaze Duthiers had shown that in Thecidium the embryo was composed of four segments with eye-spots and other strong articulate features. Fritz Müller had given a description, with figures, of the early stage of *Discina*, in which we have not only little cirri projecting from the shell, but a little appendage recalling the plug or operculum in some of the tubiculous worms.

Of great importance also was the fact that in the early stage of *Discina*, Müller observed large bristles, and these were moved freely by the animal. Smitt had shown that in certain Polyzoa (*Lepralia*) the embryo, besides being furnished with cilia, also supported several bristles or setæ which were locomotive, and finally in the worms, Claparède and Mecznikow had figured an embryo of *Nerine* in which barbed bristles were also developed. Mr. Morse referred to his communication before the American Association for the Advancement of Science on the early stages of the Brachiopods, in which he had shown the intimate connections existing between this group and the Polyzoa. Now Leuckart had already seen reasons for placing the Polyzoa with the Annelids, and he would call attention to *Crepina gracilis* and *Phorronis hippocrepia*, admitted to be worms or early stages of

them, and their close resemblance in nearly every point of their structure to the hippocrepian Polyzoa. Mr. Morse then stated that in the evidence already given, he had drawn his conclusions from alcoholic specimens of *Terebratula* and *Discina*, and from the papers of Lacaze-Duthiers, Claparède, Mecznikow, Hancock, Huxley, Vogt, Hyatt, Williams, De Morgan and others. He felt the importance of first examining *Lingula* in a living condition before making these announcements and for this reason he had recently visited the coast of North Carolina for the express purpose of finding if possible the rare *Lingula pyramidata* of Stimpson, first discovered by Prof. Agassiz in South Carolina. After nearly a week's fruitless search he had found it, had studied it alive, and had brought with him living examples, which he has the pleasure of exhibiting before the Society.

He would here express his deep sense of gratitude to Dr. Elliott Coues, Surgeon U. S. A. at Fort Macon, N. C., and the Commandant of the Post, Major Joseph Stewart, U. S. A., for the constant aid and sympathy rendered to Dr. A. S. Packard and himself during their visit there. He would not enter into a description of *Lingula* as he had already in preparation a memoir upon the subject, but would call attention simply to the additional evidence in support of the views advanced.

Lingula was found in a sand shoal at lowwater mark, buried just below the surface of the sand. The peduncle was six times the length of the shell, and was encased in a *sand tube* differing in no respect from the sand tubes of neighboring annelids. In many instances the peduncle was broken in sifting them from the sand, yet the wound was quickly repaired, and another sand case was formed.

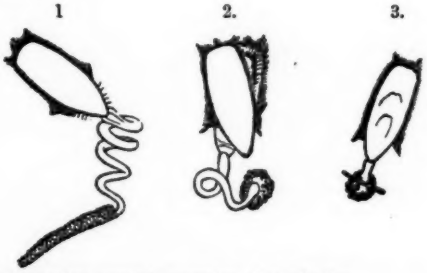


Fig. 1. Peduncle perfect, retaining portion of sand tube.
Fig. 2. Showing valves in motion; peduncle broken and forming new sand-case.
Fig. 3. Peduncle broken close to the body, and forming new sand-case.

He observed that *Lingula* had the power of moving over the sand by the sliding motion of the two valves, using at the same time the fringes of setæ which swung promptly back and forth

like a galley of oars, leaving a peculiar track in the sand. In the motion of the setæ he noticed the impulse commencing from behind and running forward.

Within the mantle he found a series of rows of prominent lamellæ in which the blood rapidly circulated, thus confirming the correctness of Vogt's observations. These lamellæ were contractile, however.

The peduncle was hollow, and the blood could be seen coursing back and forth in its channel. It was distinctly and regularly constricted or ringed, and presented a remarkably worm-like appearance. It had layers of circular and longitudinal muscular fiber, and coiled itself in numerous folds or unwound at full length. It was contractile also, and would quickly jerk the body beneath the sand. But the most startling observation in connection with this interesting animal, was the fact that its blood was *red*. This was strongly marked in the gills and various ramifications of the mantle and in the peduncle. At times the peduncle would become congested and then a deep rose blush was markedly distinct. Mr. Morse expressed his gratification in having come to the conclusions in regard to the annelidan characters of Brachiopods a long time previous to his observations on *Lingula*.

He then concluded by stating that the Brachiopods, with the Polyzoa, should be removed from the Mollusca, and placed with the Articulates among the Annelids. That the Brachiopods came near the tubiculous worms, though they were much more highly cephalized. That they exhibit certain crustacean characters, but were widely removed from the Mollusca unless a relation could be traced through the homologues of the Polyzoa to that aberrant group the Tunicates, as pointed out by Allman. He believed the Brachiopods to be a comprehensive type, exhibiting general articulate features, and forming another example of those groups belonging to the last that exhibit the characters of two or more classes combined.

It was interesting in this connection to remark that Lingula, one of the earliest forms created, had yet remained the same through all ages of the earth's history.

A New Comet.—WINNECKE discovered a new telescopic comet at Carlsruhe on the night of the 29–30 of May. The position obtained by him for May 30, is as follows: M. T. at C. $14^h\ 13^m\ 34^s$. R. A. $0^h\ 50^m\ 9{\cdot}55^s$. Decl. $+28°\ 52'\ 18''$. Vogel found for the same comet at the Leipsic observatory, M. T. at Leipsic $13^h\ 2^m\ 28{\cdot}5^s$, R. A. $0^h\ 50^m\ 4{\cdot}09^s$. Decl. $+28°\ 53'\ 17{\cdot}4^s$. We would call the attention of American astronomers to the prizes for the discovery of comets offered by the Austrian Academy. (See this Jour., vol. xlix, p. 442).

SCIENTIFIC INTELLIGENCE.

I. PHYSICS AND CHEMISTRY.

1. *On the emission, absorption and reflexion of varieties of heat radiated at low temperatures.*—MAGNUS has communicated an interesting and valuable paper on the heat radiated at low temperatures, and on the absorption and reflexion of such rays. The results of this investigation,—the last we suppose made by the lamented author,—are so far as published, in his own words as follows:

(1.) Different bodies at 150° C. radiate different kinds of heat. These kinds of heat are more absorbed by a substance of the same kind, as the radiating body, than by others, and this absorption increases with the thickness of the absorbent.

(2.) There are substances which emit only one or a few kinds of heat, others, which emit many kinds.

(3.) To the first of these belongs rocksalt when quite pure. Just as its ignited vapor, or that of one of its constituents, sodium, radiates but one color, so rocksalt, even at a low temperature, emits but one kind of heat. It is monothermic, as its vapor is monochromatic.

(4.) Rocksalt even when quite clear emits, together with its peculiar rocksalt-heat, heat which is not more absorbed by a plate of rocksalt 80^{mm} in thickness than by one 20^{mm} in thickness.

(5.) Rocksalt absorbs very powerfully the heat which it radiates. It therefore does not, as Melloni supposed, allow all kinds of heat to pass through it with equal facility.

(6.) The great diathermancy of rocksalt does not depend upon its less power of absorption for different kinds of heat, but upon the fact that it radiates only one kind of heat, and consequently absorbs only this one, and that almost all other substances send out heat containing only a small fraction or none of the rays which rocksalt emits. But all rays which differ from those radiated by any substance, are not absorbed by it, but pass through with undiminished intensity.

From this we may infer that every substance is diathermanous, only because it radiates but few waves of quite definite length, and consequently absorbs only these, allowing all the others to pass through.

(7.) Sylvin behaves like rocksalt, but is not monothermic to the same extent. In the case of this substance also an analogy exists with its ignited vapors, or those of potassium, which as is well known yield a nearly continuous spectrum.

(8.) Fluor spar completely absorbs pure rocksalt heat. We ought therefore to expect that the heat which it emits will be equally absorbed by rocksalt. Nevertheless 70 per cent of this heat pass through a rocksalt plate 20^{mm} in thickness. This may doubtless be easily explained with reference to the proportion of the quantity of heat which fluorspar emits in comparison with that

of the rocksalt; still it is possible that fluorspar at 150° emits rays other than those which it absorbs at ordinary temperatures. This behavior is however probably connected with the great reflecting power of fluorspar for rocksalt-heat, of which I shall speak in the second part of this memoir.

(9.) If it were possible to produce a spectrum of the heat radiated at 150° C., the spectrum would, if rocksalt were the radiating body, exhibit only one luminous band. If sylvin were used as a radiator, the spectrum would be much more extended, but would still occupy but a small portion of the spectrum which the heat radiated from lamp-black would form.

In concluding this part of his memoir Magnus makes some remarks on transparency which seem to us very suggestive. If we assume that there is a constant interchange of heat even between bodies having the same temperature, we may fairly assume also that there is such an exchange in the case of light. We cannot observe the light which bodies emit at ordinary temperatures, but they do not absorb light, since this absorption produces their colors. If such an exchange of light takes place at ordinary temperatures, it would follow that transparent bodies either radiate only such rays as are not contained in the light emitted by ignited bodies, and then they absorb none of these rays, or that they emit only one or a few of the wave-lengths of the light which is visible to us; since then they absorb only these, and allow all others to pass through, so that the intensity of the transmitted light is but little less than that of the incident light. We may therefore infer that the transparency of bodies depends upon the fact that they only radiate a few of the wave-lengths, which are contained in the light known to us.—*Pogg. Ann.*, Band cxxxix, p. 431. W. G.

2. *On the breadth of the spectral lines.*—Lippich has given an explanation of the breadth of the spectral lines of luminous gases, which depends upon the now generally received dynamical theory of gases and vapors. The explanation amounts to a mathematical theory of the broadening of the bands, the diminutions in their intensity, the addition of new bands, and the appearance of parts of continuous spectra, all of which appear to depend upon changes of temperature. In the present paper the author treats only of the application of the theory to the widening of the bands. The fundamental assumption is this; if it be necessary to consider a molecule of a gas as a system capable of vibration, the spectrum of an ideal gas in which the molecules would be perfectly free elastic systems, could consist only of a number of different colored bands of absolutely homogeneous light, if the vibratory motions of the molecules alone are taken into account. According to the theory of Krönig and Clausius, the molecules of a gas have progressive motions, with very great velocities, and this circumstance in connection with the well known influence of the motion of a luminous point upon the refrangibility of the emitted rays, makes it possible to explain the widening of the spectral

bands, and to show the dependence of the breadth of the brands upon the temperature and density of the ignited gas. Setting out with these principles, the author arrives at the following law:

The ratio of the difference of the wave-lengths which correspond to the borders of a spectral band, to the mean wave-length of this band, is for one and the same gas constant for all the spectral bands, and in different gases is directly proportional to the square root of the absolute temperature, but inversely proportional to the square root of the density.

The breadths of the bands, as they appear in the spectroscope to the eye, will increase with the refrangibility of the rays somewhat more rapidly than $\frac{1}{\lambda^2}$. The author in the next place shows that in the case of dark bands in a spectrum, produced by inversion, the distribution of the darkness, so to speak, and the breadths of the band will follow the same law as that above given for bright bands. The mere comparison of the relative breadths of the bands may lead to important conclusions. If in any gas-spectrum bands of different breadths lie near together, we might infer the presence of a mixture of gases of different densities, or of different allotropic states of the same gas. Thus the author thinks that the presence of some fine lines in the blue of the oxygen-spectrum may indicate the presence of the denser ozone. Furthermore, the observation of the breadths of these lines may decide whether the appearance of new lines is due to the more intense vibrations of the same molecules, or to their allotropic conditions. For the same gas the breadth of the spectral bands permits a conclusion as to the temperature. This is of special interest in the case of the heavenly bodies, in the spectra of which a widening especially of the hydrogen lines has been observed. The author does not agree with Frankland and Lockyer in ascribing this result solely to the higher pressure of the gas, but thinks that it must also have a much higher temperature. As Huggins and Lockyer have observed a widening of certain dark lines in the spectrum of the umbra of a solar spot, the author infers that the gases and vapors forming the umbra must have a higher temperature than that of the part of the sun's atmosphere, in which Fraunhofer's lines originate. Finally, Lippich suggests that measurements of the breadths of the spectral bands will finally lead not merely to relative but to approximate absolute values of temperature and density. Among other results, it may be possible in this way to furnish an experimental proof of the correctness of the dynamical theory of gases. In conclusion, the author remarks that, strictly speaking, the results which he has obtained apply only to perfect gases. Changes in the spectrum will indicate changes from gases to vapors. New periods of vibration will occur, which will exhibit themselves in the spectrum with the less intensity, the greater their deviation from the periods of vibration of the molecules of the perfect gas. In this manner a spectral band will fade out on both sides, and with increased pressure the illuminated portion will become broader the more the gas deviates from the law of

Gay Lussac and Mariotte. This explanation agrees with Wüllner's experiments, according to which the "washing out" of the bands occurs in the cases of oxygen and nitrogen, under much lower pressures than in the case of hydrogen.—*Pogg. Ann.*, cxxxix, p. 465. W. G.

3. *On the position of thallium among the elements.*—RAMMELSBERG has contributed some interesting facts in relation to the compounds of thallium, without however furnishing data for determining the degree of atomicity of the metal. The author, in the first place, calls attention to the fact that the isomorphism of thallium in its compounds with potassium and sodium is not sufficient to decide the question. When thallic sesquioxyd is heated with a solution of iodic acid, a normal dithallic salt is formed, which has the formula $\mathrm{Tl(IO_3)_6}+3\mathrm{aq}$. The sesquioxyd dissolves easily and completely in chlorhydric acid, forming a colorless solution, which after addition of potassic or ammonic chlorid yields beautiful crystals. The new salts, respectively $\mathrm{TlCl_6,6KCl}+4\mathrm{aq}$. and $\mathrm{TlCl_6,6NH_4Cl}+4\mathrm{aq}$., form large colorless transparent crystals, which resemble combinations of the cube, octahedron and dodecahedron, but which really belong to the square prismatic system. They are not decomposed by water, even on boiling. With bromine and potassic bromid, and iodine and potassic iodid, thallium forms the salts, $\mathrm{TlBr_6,3KBr}+3\mathrm{aq}$. and $\mathrm{TlI_6,3KI}+3\mathrm{aq}$., which crystallize in regular octahedra. Rammelsberg remarks that the thallium atom, Tl=204, can hardly be regarded as other than monatomic, but as the double atom of the molecule in the dithallic compounds is hexatomic, the single atom would have to be considered as tetratomic. The specific heat of thallium is an evidence that the metal is Tl=204, but the isomorphism of two monatomic with one diatomic atom has been established in so many cases that no certain conclusion can be drawn from the isomorphism of thallium with potassium and sodium. It seems probable that the question can only be settled by determinations of vapor density.—*Berichte der Deutschen Chemischen Gesellschaft, Jahrgang*, 3, No. 7, p. 360. W. G.

4. *On a new method for the volumetric estimation of copper.*—WEIL has given a new method for the determination of copper which appears deserving of attention. It is based upon the following facts. At a boiling heat and in presence of an excess of free chlorhydric acid the least trace of cupric chlorid communicates a very distinct greenish yellow tint to the solution. This tint is the more intense the greater the quantity of acid present. Stannous chlorid instantly reduces under these circumstances cupric chlorid to colorless soluble cuprous chlorid.

$$2\mathrm{CuCl_2}+\mathrm{SnCl_2}=\mathrm{Cu_2Cl_2}+\mathrm{SnCl_4}.$$

The reaction is finished when the green solution of cupric chlorid is completely decolorized. A single drop of stannous chlorid in excess is easily detected by a drop of mercuric chlorid, which gives a precipitate of calomel. When the solution contains iron in the form of sesquioxyd as well as copper, the volume of the

solution of tin employed will indicate the sum of the copper and iron. In this case the author precipitates the copper in another portion of the assay by means of zinc coupled with platinum, and then determines the iron by means of potassic hypermanganate. The copper is then easily found by difference. The author determines the litre of his solution of tin by means of pure metallic copper, and preserves it from oxydation under a layer of petroleum. It is of course necessary that the solution of copper should be perfectly free from nitric acid.—*Comptes Rendus*, lxx, p. 997.

W. G.

5. *On the utilization of the secondary products obtained in the manufacture of chloral.*—Dr. A. W. Hofmann has examined a mixture of secondary products obtained during the manufacture of chloral, and condensed during cold weather. The liquid began to boil at 17°–18°, rising slowly to 30°–31°, where the temperature remained constant a short time, and then rising again to 50°, when nearly all distilled over. The most volatile portions were mixed with three times their volume of alcohol saturated at 0° with ammonia and heated in a water bath for an hour. The liquid was then filtered to separate crystals of sal-ammoniac and the alcohol ammonia and chlorinated ethylic chlorids distilled off. The mass of chlorhydrates of ethyl-ammonias remaining were decomposed with caustic soda, and the separated liquid alkalies dehydrated by caustic soda, and finally distilled. In this manner 5 litres of the secondary products operated on gave 1½ liters of a mixture of anhydrous ethylamines. These could be separated from each other by means of oxalic ether, in the manner already pointed out by Hofmann. The results of this investigation are interesting, from the prospect which they afford of obtaining the ethyl-ammonias as an article of commerce, at a reasonable price, and in comparative abundance.—*Comptes Rendus*, lxx, p. 906. W. G.

6. *On the nature of the secondary products obtained in the manufacture of chloral.*—Krämer has studied the other products of the action of chlorine upon alcohol, the existence of a large quantity of ethylic chlorid having been shown by Hofmann. As the ethylic chlorid was in contact with an excess of chlorine, it was natural to expect to find in the less volatile oily products the whole series of chlorinated ethylic chlorids described by Regnault, and experiment showed that several of these substances were present. The most volatile product boiling at 60°, proved to be chlorinated chlorethyl or chlorethyliden, $C_2H_4Cl_2$, identical with the chlorethyliden prepared from aldehyd. A liquid boiling at 85°, proved to be ethylen-dichlorid, the formation of which by the action of chlorine upon ethylic chlorid had not before been observed. The next product was chlorinated ethylen dichlorid $C_2H_3Cl.Cl_2$ boiling at 115°, and the last bichlorinated ethylen, $C_2H_2Cl_2$, boiling at 37°. Other chlorinated products were also observed, but not yet studied. To prove the identity of the chlorethyliden obtained in this manner with that obtained from aldehyd by the action of phosphoric pentachlorid, Krämer heated a portion of it

with alcoholic ammonia to 160° for 12 hours. In this manner an oily base boiling at 180°–182°, and having the characteristic odor of collidine, $C_8H_{11}N$, was obtained. This base had already been formed from aldehyd-ammonia by Bæyer, and found to be identical with that obtained by Anderson from animal oil.—*Berichte der Deutschen Chemischen Gesellschaft, Jahrgang*, 3, p. 257–262.

W. G.

7. *On some properties of iron precipitated by the galvanic current.*—LENZ has given some interesting particulars in relation to the composition and properties of iron as precipitated in the metallic form by the battery. The iron examined was deposited by Klein's process from a solution of the mixed sulphates of iron and magnesium. Weak currents were employed, and the solution was kept neutral by carbonate of magnesia. Iron so thrown down has a beautiful fine-granular structure, showing no traces of crystals under the microscope. Its color is a soft bright gray. Its hardness is very remarkable—not less than 5·5 of the ordinary mineral scale, and it is excessively brittle, so that it may be rubbed to powder between the fingers. When the iron is slowly reduced upon a polished surface, it is free from flaws and has a velvety look. As it becomes thicker, bubbles or pits are formed as small oval depressions. When heated over a fire the iron loses many of these properties in a remarkable degree. Its hardness diminishes and becomes 4·5; its brittleness entirely disappears, and it becomes so flexible and tenacious that it cannot be broken by repeated bending or even by folding and strongly smoothing down the folds. When heated in vacuo the iron changes color and becomes almost as white as worked platinum. The ignited iron rusts very quickly both in air and in previously boiled water; this is not the case with the metal before ignition. In the electric series unignited iron stands nearer to copper than the ignited metal. On analysis by means of Sprengel's pump, the precipitated iron was found to contain various gases—vapor of water, nitrogen, carbonic oxyd, carbonic acid and hydrogen. Levy thinks that the carbonic acid came from the solution from which the iron was precipitated, and that the carbonic oxyd was formed during the ignition of the tube containing the iron in the process of analysis; also that the vapor of water was formed by the union of hydrogen with the oxygen of a small amount of rust in the iron, since it was only given off at 1,600° C. The hydrogen in the iron was always in largest quantity; the whole quantity of gas varied greatly, and sometimes amounted to 185 times the volume of the iron. The absorption of the gases was found to take place mainly in the first layers formed. On warming the reduced iron, the evolution of gas began at temperatures below 100°, but at this temperature chiefly hydrogen was evolved. Ignited galvanically reduced iron decomposes water and absorbs the free hydrogen either wholly or partially.—*Bulletin de l' Académie de Pétersbourg*, xiv, p. 337, *cited in Dingler's Polytechnisches Journal*, cxcvi, p. 44. W. G.

8. *On the preparation of barium chlorate.*—BRANDAU has proposed the following simple method for the preparation of barium chlorate: Commercial crystallized aluminum sulphate, sulphuric acid, and potassium chlorate, in the ratio of one molecule of each of the two former to two of the latter, are mixed with water to the consistence of a thin paste, warmed for half an hour on the water-bath, allowed to cool completely, and treated with alcohol in excess. Upon filtering, and neutralizing with barium hydrate, barium sulphate and some aluminum hydrate are precipitated, and barium chlorate remains in solution. The alcohol is distilled off, and the filtrate on evaporation yields crystals of the pure barium chlorate. The only precaution necessary is to have the aluminum sulphate and the sulphuric acid in slight excess.—*Ann. Ch. Pharm.*, cli, 361, Sept. 1869. G. F. B.

9. *On the properties of Selenium.*—RATHKE has investigated, in the Halle laboratory, the various modifications which selenium, like sulphur, is capable of assuming. Notwithstanding the difference in the behavior of the various forms of each, respectively, to solvents, especially CS_2, Rathke places the insoluble black selenium with the rhombic variety of sulphur, and the red amorphous selenium with the insoluble amorphous form of sulphur. As in the case of sulphur, the latter variety of selenium is produced by the decomposition of selendithionates (solutions of selenium in alkaline sulphites) by acids; by the action of water on selenium chlorid; and by the sudden cooling of fused selenium. So on the other hand, by the slow decomposition of a solution of potassium selenid, distinct crystals of black selenium are produced, precisely as when by the similar decomposition of alkaline sulphids, large rhombic crystals of sulphur separate. Though red selenium is more stable than the corresponding form of sulphur, yet, like this, it passes into the other variety on raising the temperature to 100° C., with a distinct evolution of heat. The specific gravity of selenium in these forms, is as follows: For the black variety, 4·80 to 4·81; the red, crystallized from carbon disulphid, (and corresponding to monoclinic sulphur,) 4·46 to 4·51, the red, amorphous, 4·26. Agreeing here, also, with the corresponding forms of sulphur, as already given. Regarding the behavior of selenium toward carbon disulphid as exceptional, Rathke tried the action of other solvents. Sulphur chlorid saturated with selenium with the aid of heat, deposits on cooling, crystals of sulphur, leaving selenium chlorid, in which selenium dissolves freely, forming when cold a syrupy liquid, from which, on standing, the black variety of selenium separated in small nodules. The solubility of rhombic (octahedral) sulphur in carbon disulphid, led to the supposition that selenium might be equally soluble in carbon diselenid. Various methods for preparing this substance were tried, but, though small quantities were obtained by acting on selenium phosphid with moist vapor of carbon tetrachlorid, heated in a tube, sufficient for the above purpose could not be obtained. Carbon tetrachlorid itself, does not dissolve the black variety of selenium, selenium-ethyl dissolves both,

but in very small quantity. Of the solvents tried, only those which were selenium-compounds dissolved black selenium.

Rathke also examined the compounds which sulphur forms with selenium, hoping to throw some light upon their mutual iso-dimorphism. He first melted the substances together in the ratio of one atom of selenium to two of sulphur, repeatedly extracted the resulting mass with carbon disulphid, and crystallized out the portion dissolved in fractions. The crystals, however, increased progressively in sulphur and diminished in selenium. The precipitate obtained when sulphydric gas and selenium dioxyd gas act upon each other in solution, was then examined. By solution in carbon disulphid and evaporation, well-formed rhombic prisms, in color like potassium dichromate, were obtained, which gave on analysis 63·86 per cent selenium and 35·50 per cent sulphur. These crystals, then, though by no means a mixture of the constituents, do not correspond in composition to the formula SeS_2, which requires 55·42 per cent Se and 44·58 per cent S. The process was then reversed, and the precipitate produced when selenhydric gas reacts upon sulphur dioxyd collected and treated as above. It contained considerable black selenium, not taken up by the solvent. On fractionally crystallizing, three products were obtained, one (*c*) soluble in 57 parts CS_2, the second (*b*) in 263 parts, and the third (*a*) considerably more so than selenium itself. (*a*) consisted of minute dark ruby-red, rounded crystals; (*b*) of brilliant-red prisms; and (*c*) of orange-red tabular prisms. On analysis, (*a*) was found to agree nearly with the formula Se_2S; but (*b*) and (*c*) were intermediate between Se_2S and SeS_2. Rathke regards them as isomorphous mixtures.

Carbon diselenid CSe_2 was prepared but only in small quantity, not even sufficient for analysis. It appeared as a thin, brilliant-brown liquid, of peculiar and disagreeable odor, recalling at first, when dilute, that of carbon disulphid, but being when concentrated, very pungent and irritating to the eyes. It burned with the blue selenium flame, was not very volatile, and was insoluble in all solvents tried. At the same time there was formed ethyl selenxanthate, $\left.\begin{matrix} C_2H_5 \\ CSe \\ C_2H_5 \end{matrix}\right\}Se$ and potassium selenxanthate, $\left.\begin{matrix} C_2H_5 \\ CSe \\ K \end{matrix}\right\}Se$.

Rathke also also attempted the production of selenium tetra-ethyd but succeeded in getting only tri-ethylselenin chlorid $Se(C_2H_5)_3Cl$, in combination with zinc chlorid, and also with platinum chlorid.—*Ann. Ch. Pharm.*, clii, 181, Nov. 1869. G. F. B.

10. *On the preparation of nitrogen pentoxyd (nitric anhydrid.)*—Odet and Vignon employ for this purpose a double U tube, in each bend of which 140 to 150 grams of silver nitrate is placed, the whole being heated to a temperature of 60° C. Phosphoryl chlorid ($POCl_3$) is allowed to fall drop by drop, into the first leg of the tube. The nitryl chlorid (NO_2Cl) thus produced, reacts upon the silver nitrate in the second bend, to which is attached, by melting, a receiver, in which the product of the reaction collects.

This receiver is immersed in a freezing mixture. No disengagement of oxygen takes place, silver phosphate and chlorid being the only secondary products. The reactions are as follows:—

$$(1)\quad \left.\begin{matrix}N\Theta_2\\ Ag\end{matrix}\right\}\Theta\Big)_3 + (P\Theta)\,Cl_3 = \left.\begin{matrix}P\Theta\\ Ag_3\end{matrix}\right\}\Theta_3 + (N\Theta_2Cl)_3.$$

$$(2)\quad \left.\begin{matrix}N\Theta_2\\ Ag\end{matrix}\right\}\Theta + (N\Theta_2)Cl = \left.\begin{matrix}N\Theta_2\\ N\Theta_2\end{matrix}\right\}\Theta + AgCl.$$

—*Comptes Rendus*, lxix, 1142. G. F. B.

11. *On a new method for preparing bromhydric acid.*—Champion and Pellet describe a new and simple mode of preparing bromhydric acid by the action of bromine upon paraffin at a moderately elevated temperature. Two retorts are employed, one for the bromine, the other for the paraffin. The neck of the first retort is prolonged and bent at right angles so as to enter the tubulure of the second—being fixed there by a ground joint—terminating near the bottom. By means of a sand or oil-bath, the paraffin is kept at a temperature of 180° C. and by a saline bath the bromine is maintained at 65° C. As the bromine gradually distils over into the paraffin, the bromhydric acid gas is evolved, and after passing through a bulb tube containing water, and a U tube filled with broken glass and bits of moistened phosphorus, to remove any traces of unchanged bromine, it is collected in the liquid to be saturated with it, placed in a vessel surrounded with ice. The aqueous solution thus obtained saturated at 0° C. has a density of 1·78, and corresponds to the formula $HBr,\ H_2\Theta$. Each c. c. contains 1·46 grms. HBr.—*Bull. Soc. Ch.*, II, xiii, 197, March, 1870.

G. F. B.

12. *On the recovery of Uranium from the Phosphate.*—In using a solution of uranium for determining volumetrically phosphoric acid, residues of uranium phosphate are obtained, from which it is desirable to recover the uranium. Two methods for doing this have been lately proposed. The first by Heintz, obtains the uranium as nitrate. The phosphate, previously washed, dried and weighed, is dissolved in nitric acid. Half as much pure tin is weighed out, and nine-tenths of it added to the nitric solution, which is then heated until the tin is entirely converted into stannic hydrate; if, in this solution, ammonia gives a precipitate not entirely soluble in acetic acid, the rest of the tin is added, and the process repeated. All the phosphoric acid is contained in the precipitate. The filtrate is diluted, treated with sulphydric gas to precipitate the last traces of tin, again filtered, and evaporated to crystallization.

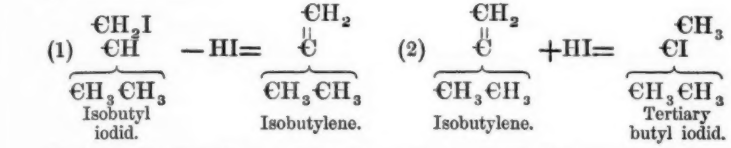
The second method, proposed by Reichardt, consists in dissolving the uranium residues in either nitric or chlorhydric acid, adding an excess of ferric chlorid, making the solution acid with acetic acid by adding sodium acetate, and after considerable dilution, heating to boiling. All the phosphoric acid, together with the excess of iron, is thus precipitated. Instead of boiling the solution after the addition of the ferric chlorid, it may be treated with

an excess of sodium carbonate, and filtered. In presence of free carbonic acid, the sodium carbonate holds the uranium in solution, the phosphoric acid being precipitated with the iron. The filtrate from either of these methods is acidulated with chlorhydric acid, and after boiling to expel the carbonic gas, the uranium oxyd is precipitated with ammonia.—*Ann. Ch. Pharm.*, cli, 216, Aug. 1869. *Zeitschr. analyt. Chem.*, 1869, 116. G. F. B.

13. *On the modifications of sulphur trioxyd (sulphuric anhydrid.)*—SCHULTZ-SELLACK has examined the isomeric modifications of sulphur trioxyd. It was prepared by distillation from di-sulphuric acid (fuming,) the vapors being rendered anhydrous by passing them over phosphoric anhydrid. On cooling the liquid thus obtained, the thermometer immersed in it is observed to become stationary at 16° C., the fluid solidifying in long transparent prisms. These melt again at the same temperature, the liquid being obtained again, frequently unaltered. Sometimes, however, white flocks remain in the melted mass, which gradually collect in warty masses of fine white needles, until the entire liquid has become a matted mass of them. The same change takes place when the liquid is kept for some time at a temperature below 25° C.; above 27° C., however, it does not take place. The solid anhydrid thus produced becomes gradually fluid again when the temperature rises to above 50° C.; but this solidification and liquefaction is not a freezing and melting, in the ordinary sense of these terms, since both take place gradually, within certain definite ranges of temperature. The fluid anhydrid has a remarkably high coefficient of expansion by heat; being for temperatures between 25° and 45°, 0·0027 for each degree, more than two-thirds that of the gases. It boils at 46° C. under a pressure of 760^{mm}·; at 20° its vapor-tension is 200^{mm}. In a vacuum the first modification shows no vapor-tension; but gradually vapor is evolved, so that after several days, a tension of 30 or 40^{mm}· at 20° is obtained. The vapor of the solid as well as that of the fluid trioxyd has a normal density, found to be 2·74 to 2·76; $S\Theta_3$ requires 2·76. From the above facts, the author concludes that sulphur trioxyd exists in two states: 1st, α Sulphuric anhydrid solidifying at +16° in long colorless prisms, which melt at the same temperature; boiling at 46° C. 2d. β Sulphuric anhydrid, produced from the former, at temperatures below 25°, in very fine needles; becoming at temperatures above 50° gradually fluid again, being transformed into α; yielding vapor very slowly at ordinary temperatures, which is like that given by α, but has a less tension. He regards β as a polymer of α, since many polymeric organic compounds, as, for example, cyanuric acid, show similar variations in physical properties, as their molecules are more condensed.—*Ber. Berl. Chem. Ges.*, iii, 215, March, 1870. G. F. B.

14. *On the conversion of isobutyl alcohol into tertiary pseudo-butyl alcohol.*—From purely theoretical considerations, MARKOWNIKOFF was led to believe that isobutyl alcohol (that produced during fermentation) by the loss of water, or its iodid by the loss of

hydriodic acid, would give a butylene identical with the isobutylene of Butlerow. By heating isobutyl iodid with alcoholic potash, a butylene was obtained, which united directly with hydriodic acid, giving a butyl iodid having the boiling point of tertiary pseudo-butyl iodid. On treating this with moist silver oxyd, tri-methyl-carbinol (tertiary pseudo-butyl alcohol) was obtained with its very characteristic properties. The reaction which takes place is as follows:



Morkownikoff believes that these facts sustain his general law of the formation of unsaturated compounds; a law which for the homologues of ethylene may thus be formulated: *When water is removed from an alcohol-molecule, that carbon-atom which is directly united with the carbon-atom holding the hydroxyl, furnish the hydrogen.* A similar law holds in the case of the haloid anhydrids of the monatomic alcohols. *When combination takes place between an unsymmetrically constituted homologue of ethylene* (for example, propylene $ĊH_3ĊHĊH_2$) *and water* (*or an haloid acid*), *the residues into which these latter bodies are separated, are divided between the two carbon-atoms so that the hydroxyl* (*or the corresponding halogen*), *is united to the least hydrogenized carbon-atom in the compound.*—*Zeitschr. Chem.*, II, vi, 29, Dec., 1869. G. F. B.

15. *On the Synthesis of Aromatic Acids.*—Starting from the well known facts that a monobasic acid may be viewed as a hydrocarbon in which an atom of hydrogen is replaced by carboxyl (ĊOOH), and further, that, in general, the basicity of acids depends upon the number of carboxyl groups which they contain, Wurtz proposes to make practical use of them in synthesis. By effecting one or more such replacements in a hydrocarbon, either by the carboxyl group itself or its ethyl-derivative ($ĊOO(Ċ_2H_5)$), —which can be effected by heating the corresponding bromine derivative of the next lower homologous hydrocarbon with ethyl chlorocarbonate and sodium-amalgam,—any desired aromatic acid may be produced. In this way, for example, Wurtz prepared benzoic acid; 90 grams monobromo-benzol (phenyl bromid, $Ċ_6H_5$ Br) being heated with 60 grams ethyl chlorocarbonate ($ĊOO(Ċ_2H_5)Cl$) and 3·5 kilograms sodium-amalgam containing one per cent of sodium. The action proceeded slowly, requiring many days for completion, even at 110° C. Carbonic and carbonous gases were evolved, sometimes mixed with a gas which burned with a green flame, probably ethyl chlorid. When the fluid had entirely disappeared and the mercury had again recovered its mobility, the saline mass was extracted with ether, and the ether distilled off till the temperature rose to 150° C. The residue, which

contained ethyl benzoate, on being treated with potassium hydrate, evaporating to dryness to remove the alcohol, re-solution in water, and precipitation with hydrochloric acid, afforded benzoic acid abundantly. The reaction, therefore, is as follows:

$$\mathrm{C_6H_5Br + CO\left\{\begin{matrix}OC_2H_5\\Cl\end{matrix}\right. + Na_2 = NaCl + NaBr + C_6H_5(COOC_2H_5)}$$

By using mono-bromo-toluol $\left(\mathrm{C_6H_4\left\{\begin{matrix}CH_3\\Br\end{matrix}\right.}\right)_1$ toluic acid $\left(\mathrm{C_6H_4\left\{\begin{matrix}CH_3\\COOC_2H_5\end{matrix}\right.}\right)$ with a trace of another acid perhaps isomeric with it, was obtained. With the isomeric benzyl bromid ($C_6H_5(CH_2Br)$) the reaction yields a more complicated product.

In a more recent paper, Wurtz shows that the acid mentioned above as obtained simultaneously with the toluic, is iso-toluic acid; and that its presence is due to the fact that the bromo-toluol used, contained an isomeric body. The substance obtained by acting upon benzyl bromid or chlorid, he finds to have the composition $C_{15}H_{14}O_2$, and he gives it the name di-benzyl-carboxylic acid. In a second operation, 252 grams benzyl chlorid, 108 grams ethyl chlorocarbonate and 8,000 grams one per cent sodium-amalgam, were heated on a saline bath, with an upward condenser, till the whole mass was solid. This residue was extracted with ether, the ether distilled off till the temperature rose to 180°, the fluid remaining in the retort decomposed with alcoholic potash, the the alcohol evaporated, the residue dissolved in water, precipitated by hydrochloric acid and recrystallized from water. It separated in drops which solidified to a mass of fine needles. It is almost insoluble in cold water, and but little soluble in hot; alcohol and ether dissolve it readily. At 84° it melts and at a higher temperature distils. Its vapors are aromatic and irritating. To produce it, Wurtz assumes that under the influence of the sodium, the chlorid, by the loss of hydrochloric acid, becomes chloro-di-benzyl $\mathrm{C_6H_5CH_2}$ / $\mathrm{C_6H_5CHCl}$ and that this by the simultaneous action of the sodium and the ethyl chlorocarbonate becomes ethyl di-benzyl-carboxylate $\mathrm{C_6H_5CH_2}$ / $\mathrm{C_6H_5CHCOO(C_2H_5)}$. —*Comptes Rendus*, lxviii, 1298; lxx, 350.

G. F. B.

16. *On the volatile acids of Croton oil.*—Froelich having observed in the Jena laboratory, that by the action of phosphorus pentachlorid upon ethyl-diacetic acid, two metameric chlor-acids were obtained, yielding when treated with sodium-amalgam, two metameric acids of the composition $C_4H_6O_2$, of which one was solid and identical with that prepared from allyl cyanid, the other was fluid, and supposed to be the same as that described by Schlippe as occurring in Croton oil, and called Crotonic acid, Geuther undertook a confirmation of this supposition. Having prepared the volatile acids from four pounds of Croton oil, he finds that no volatile acid of the composition $C_4H_6O_2$ exists in this oil, and that the solid acid contained in it is not angelic acid; and there-

fore, that Schlippe's statements are entirely erroneous. The volatile fluid acids are essentially acetic, butyric and valeric, mixed perhaps with traces of oenanthic acid and higher members of the oleic series. The solid acid supposed by Schlippe to be angelic acid, is a metamer of it, to which Genther gives the name Tiglic acid. It constitutes more than a third of the volatile acids of Croton oil, and forms a barium salt readily soluble in water, crystallizing in pearly plates, and having the composition $\mathrm{C_5H_7Ba'O_2+5H_2O}$. It has a remarkably close correspondence in properties with the methyl-crotonic acid of Frankland and Duppa. It is therefore obvious that the name "crotonic" given to the acid $\mathrm{C_4H_6O_2}$, is a misnomer, since croton oil contains no acid of this composition. Geuther therefore proposes to call the chlor-acid of Froelich, mentioned above, which fuses at 59·5° and boils at 194·8°, monochlor-quartenic acid, and the acid derived from it by the action of sodium-amalgam, which is fluid at 15° and boils at 171·9°, quartenic acid. For the metamer of the chlor-acid, melting at 94° and boiling between 206°–211° with partial decomposition, he proposes the name mono-chlor-tetracrylic acid; and for its derivative $\mathrm{C_4H_6O_2}$, first prepared from allyl cyanid and till now called crotonic acid, the name tetracrylic acid. Its aldehyd called croton-aldehyd by Kekulé, would therefore be tetracryl-aldehyd.—*Zeitschr. Chem.*, II, vi, 26, Dec. 1869. G. F. B.

17. *On the Rhenish creosote from beech-wood tar.*—Under the direction of Baeyer and Graebe in Berlin, Marasse has made an investigation of beech-wood creosote, with results far more satisfactory and conclusive than had been previously obtained. The material on which he worked came from the manufactory of Dietze & Company in Mayence; it was colorless, a little thick, heavier than water, in which it was scarcely soluble, and dissolved completely in potassium hydrate solution. On subjecting it to fractional distillation, three separate products were obtained: one boiling below 199°, one between 200° and 203° (by far the larger portion) and one between 216° and 220°. After drying the lightest product, and subjecting it to sixteen fractional distillations, a body was obtained, which boiled between 183° and 184°, solidified on cooling, and had the properties of phenol, which an analysis proved it to be. On distilling the second and largest fraction with zinc-dust and purifying and fractioning the distillate, two products were obtained; the one, boiling between 110° and 112°, proved on analysis to be toluol, $\mathrm{C_7H_8}$, or $\mathrm{C_6H_4}\left\{\begin{matrix}\mathrm{CH_3}\\ \mathrm{H}\end{matrix}\right.$. Since the zinc-dust acts by reducing hydroxyl to hydrogen, the body yielding this toluol must have been $\mathrm{C_6H_4}\left\{\begin{matrix}\mathrm{CH_3}\\ \mathrm{OH}\end{matrix}\right.$ or cresol. The other portion boiling at 150° to 155° afforded the properties and composition of anisol. As this anisol $\mathrm{C_6H_4}\left\{\begin{matrix}\mathrm{H}\\ \mathrm{OCH_3}\end{matrix}\right.$ does not exist in the creosote as such, it must have been produced by a similar action of the zinc-dust, from the body $\mathrm{C_6H_4}\left\{\begin{matrix}\mathrm{OH}\\ \mathrm{OCH_3}\end{matrix}\right.$ which is guaiacol, the acid methyl

ether of pyrocatechin $C_6H_4\left\{\begin{matrix}OH\\OH\end{matrix}\right.$. That the fraction boiling between 200° and 203° was thus composed, Marasse further proved by fusing it with potassium hydrate. Two liquids were thus obtained which on examination proved to be cresol itself $C_6H_4\left\{\begin{matrix}CH_3\\OH\end{matrix}\right.$, and pyro-catechin $C_6H_4\left\{\begin{matrix}OH\\OH\end{matrix}\right.$, the latter produced by the saponification of its ether, guaiacol, in the experiment. The same result was reached by acting upon this fraction with hydriodic acid; cresol and pyro-catechin being produced as before. And finally, by acting upon this fraction with methyl iodid and potassium hydrate in sealed tubes, the methyl ethers of both cresol (cresyl-anisol $C_5H_4\left\{\begin{matrix}CH_3\\OCH_3\end{matrix}\right.$) and guaiacol ($C_6H_4\left\{\begin{matrix}OCH_3\\OH\end{matrix}\right.$) were obtained. The last fraction, boiling between 217° and 220°, afforded, after reduction with hydriodic acid and fractioning, phlorol, $C_8H_{10}O$ or $C_6H_3\left\{\begin{matrix}CH_3\\CH_3\\OH\end{matrix}\right.$ and homo-pyro-catechin $C_6H_3\left\{\begin{matrix}CH_3\\OH\\OH\end{matrix}\right.$, which last substance was derived from creosol, its acid methyl ether $C_6H_3\left\{\begin{matrix}CH_3\\OCH_3\\OH\end{matrix}\right.$ precisely as pyro-catechin was in the previous fraction, from guaiacol. Marasse hence concludes that Rhenish beech-wood creosote is a mixture of compounds belonging to two parallel series, the phenols and the acid methyl ethers of pyro-catechin and its homologues. And since the first members of the series do not coincide in boiling point, the first member of the guaiacol series agreeing with the second member of the phenol series, it is obvious that that portion of creosote which boils at the lowest temperature will consist of the first member of the phenol series, i. e., phenol itself.

Phenol Series.	Boiling point.	Guaiacol Series.	Boiling point.
Phenol, $C_6H_5(OH)$	184° C.	----------	----
Cresol, $C_6H_4\left\{\begin{matrix}CH_3\\OH\end{matrix}\right.$	203°	Guaiacol, $C_6H_4\left\{\begin{matrix}OH\\OCH_3\end{matrix}\right.$	200° C.
Phlorol, $C_6H_3\left\{\begin{matrix}CH_3\\CH_3\\OH\end{matrix}\right.$	220°	Creosol, $C_6H_3\left\{\begin{matrix}CH_3\\OCH_3\\OH\end{matrix}\right.$	219°

All the different kinds of beech-wood creosote appear to be identical in composition; those specimens having the highest boiling point, which contain the higher members of these parallel series.—*Ann. Ch. Pharm.*, clii, 59, Oct., 1869. G. F. B.

16. *On Ocean Currents, in relation to the Distribution of Heat over the Globe*, by James Croll of the Geological Survey of Scotland. (Phil. Mag., Feb. 1870.)—I. *The absolute Heating-power of Ocean-currents.*—* * * From an examination of the published sections [of the Gulf Stream] some years ago,* I came to the conclu-

* Philosophical Magazine for February, 1867, p. 127.

sion that the total quantity of water conveyed by the stream is probably equal to that of a stream 50 miles broad and 1000 feet deep,* flowing at the rate of four miles an hour, and that the mean temperature of the entire mass of moving water is not under 65° at the moment of leaving the Gulf. I think we are warranted to conclude that the stream, before it returns from its northern journey, is on an average cooled down to at least 40°; consequently it loses 25° of heat. Each cubic foot of water, therefore, in this case carries from the tropics for distribution upwards of 1500 units of heat, or 1,158,000 foot-pounds. According to the above estimate of the size and velocity of the stream, 5,575,680,000,000 cubic feet of water are conveyed from the Gulf per hour, or 133,816,320,000,000 cubic feet daily. Consequently the total quantity of heat transferred from the equatorial regions per day by the stream amounts to 154,959,300,000,000,000 foot-pounds.

From observations made by Sir John Herschel and by M. Pouillet on the direct heat of the sun, it is found that, were no heat absorbed by the atmosphere, about 83 foot-pounds per second would fall upon a square foot of surface placed at right angles to the sun's rays.† Mr. Meech estimates that the quantity of heat cut off by the atmosphere is equal to about 22 per cent of the total amount received from the sun. M. Pouillet estimates the loss at 24 per cent. Taking the former estimate, 64·74 foot-pounds per second will therefore be the quantity of heat falling on a square foot of the earth's surface when the sun is in the zenith. And were the sun to remain stationary in the zenith for twelve hours, 2,796,768 foot-pounds would fall upon the surface.

It can be shown that the total amount of heat received upon a unit surface on the equator during the twelve hours from sunrise till sunset at the time of the equinoxes is to the total amount which would be received upon that surface, were the sun to remain in the zenith during those twelve hours, as the diameter of a circle to half its circumference, or as 1 to 1·5708. It follows, therefore, that a square foot of surface on the equator receives from the sun at the time of the equinoxes 1,780,474 foot-pounds daily, and a square mile 49,636,750,000,000, foot-pounds daily. But this amounts to only $\frac{1}{3121\frac{1}{870}}$ part of the quantity of heat daily conveyed from the tropics by the Gulf-stream. In other words, the Gulf-stream conveys as much heat as is received from the sun by 3,121,870 square miles at the equator. The amount thus conveyed is equal to all the heat which falls upon the globe within 63 miles on each side of the equator. According to calculations made by Mr. Meech,‡ the annual quantity of heat received by a unit surface on the frigid zone, taking the mean of the whole zone, is $\frac{5\cdot45}{12}$ of

* The Gulf-stream at the narrowest place examined by the Coast Survey, and the place where its velocity was greatest, was found to be over 30 statute miles broad and 1950 feet deep. But we must not suppose this represents all the warm water which is received by the Atlantic from the equator; a great mass of water flows into the Atlantic without passing through the Straits of Florida.

† Trans. of Roy. Soc. of Edinb., vol. xxi, p. 57. Phil. Mag., S. 4, vol. ix, p. 36.

‡ Smithsonian Contributions to Knowledge, vol. ix.

that received at the equator; consequently the quantity of heat conveyed by the Gulf-stream in one year is equal to the heat which falls on an average on 6,873,800 square miles of the arctic regions. The frigid zone or arctic regions contain 8,130,000 square miles. There is actually, therefore, nearly as much heat transferred from tropical regions by the Gulf-stream as is received from the sun by the entire arctic regions, the quantity conveyed by the stream to that received from the sun by those regions being as 15 to 18.

But we have been assuming in our calculations that the percentage of heat absorbed by the atmosphere is no greater in polar regions than it is at the equator, which is not the case. If we make due allowance for the extra amount absorbed in polar regions in consequence of the obliqueness of the sun's rays, the total quantity of heat conveyed by the Gulf-stream will probably nearly equal the amount received from the sun by the entire arctic regions.

If we compare the quantity of heat conveyed by the Gulf-stream with that conveyed by means of aërial currents, the result is equally startling. The density of air to that of water is as 1 to 770, and its specific heat to that of water is as 1 to 4·2; consequently the same amount of heat that would raise 1 cubic foot of water 1° would raise 770 cubic feet of air 4°·2, or 3234 cubic feet 1°. The quantity of heat conveyed by the Gulf-stream is therefore equal to that which would be conveyed by a current of air 3234 times the volume of the Gulf-stream, at the same temperature and moving with the same velocity. Taking, as before, the width of the stream at 50 miles, and its depth at 1000 feet, and its velocity at 4 miles an hour, it follows that, in order to convey an equal amount of heat from the tropics by means of an aërial current, it would be necessary to have a current about 1¼ mile deep, and at the temperature of 65°, blowing at the rate of four miles an hour from every part of the equator over the northern hemisphere towards the pole. If its velocity were equal to that of a good sailing-breeze, which Sir John Herschel states to be about twenty-one miles an hour, the current would require to be above 1200 feet deep. A greater quantity of heat is probably conveyed by the Gulf-stream alone from the tropical to the temperate and arctic regions than by all the aërial currents which flow from the equator.

* * * * *

The anti-trades or upper return-currents, as we have seen, bring no heat from the tropical regions. After traversing some 2000 miles in a region of extreme cold they descend on the Atlantic as a cold current, and there absorb the heat and moisture which they carry to northeastern Europe. Those aërial currents derive their heat from the Gulf-stream, or if it is preferred, from the warm water poured into the Atlantic by the Gulf-stream. How, then, are these winds heated by the warm water? The air is heated in two ways, viz: by direct *radiation* from the water, and by *contact* with the water. Now, if the Gulf-stream continued a narrow and deep current during its entire course similar to what it is at the Straits of Florida, it could have little or no opportunity of com-

municating its heat to the air either by radiation or by contact. If the stream was only about 40 or 50 miles in breadth, the aërial particles in their passage across it would not be in contact with warm water more than an hour or two. Also the number of the particles in contact with the water, owing to the narrowness of the stream, would be small, and there would therefore be little opportunity for the air becoming heated by contact. The same also holds true in regard to radiation. The more we widen the stream and increase its area, the more we increase its radiating surface; and the greater the radiating surface, the greater is the quantity of heat thrown off. But this is not all; the number of aërial particles heated by radiation increases in proportion to the area of the radiating surface; consequently the wider the area over which the waters of the Gulf-stream are spread, the more effectual will the stream be as a heating-agent. And, again, in order that a very wide area of the Atlantic may be covered with the warm waters of the stream, slowness of motion is essential. * * *

The quantity of heat conveyed by the Gulf-stream, as we have seen, is equal to all the heat received from the sun by 3,121,870 square miles at the equator. Mr. Findlay, however, as has been stated, thinks that I have doubled the actual volume of the stream. Assuming that I have done so, the amount of heat carried by the stream would still be equal to all the heat received from the sun by 1,560,935 square miles at the equator. The mean annual quantity of heat received from the sun by the temperate regions per unit surface is to that received by the equator as 9·83 to 12.* Consequently the quantity of heat conveyed by the stream, taking Mr. Findlay's estimate of its volume, is equal to all the heat received from the sun by 2,062,960 square miles of the temperate regions. The total area of the Atlantic from the latitude of the Straits of Florida, 200 miles north of the tropic of Cancer, up to the Arctic Circle, including also the German Ocean, is about 8,500,000 square miles. In this case the quantity of heat carried by the Gulf-stream into the Atlantic through the Straits of Florida, to that received by this entire area from the sun, is as 1 to 4·12, or in round numbers as 1 to 4. It therefore follows that one-fifth of all the heat possessed by the waters of the Atlantic over that area, even supposing that they absorb every ray that falls upon them, is derived from the Gulf stream. Would those who call in question the efficiency of the Gulf-stream be willing to admit that a decrease of one-fourth in the total amount of heat received from the sun, over the entire area of the Atlantic from within 200 miles of the tropical zone up to the arctic region, would not sensibly affect the climate of Northern Europe? If they would not willingly admit this, why, then, contend that the Gulf-stream does not affect climate? for the stoppage of the Gulf-stream, taking it at Mr. Findlay's estimate, would deprive the Atlantic of 77,479,650,000,000,000,000 foot-pounds of energy in the form of heat per day, a quantity equal to one-fourth of all the heat received from the sun by that area.

* See Smithsonian Contributions to Knowledge, vol. ix.

Were the sun extinguished, the temperature over the whole earth would sink to nearly that of stellar space, which, according to the investigations of Sir John Herschel* and of M. Pouillet,† is not above —239° F. Were the earth possessed of no atmosphere, the temperature of its surface would sink to exactly that of space, or to that indicated by a thermometer exposed to no other heat-influence than that of radiation from the stars. But the presence of the atmospheric envelope would slightly modify the conditions of things; for the heat from the stars (which of course constitutes what is called the temperature of space) would, like the sun's heat, pass more freely through the atmosphere than the heat radiated back from the earth, and there would in consequence of this be an accumulation of heat on the earth's surface. The temperature would therefore stand a little higher than that of space; or, in other words, it would stand a little higher than it would otherwise do were the earth exposed in space to the direct radiation of the stars without the atmospheric envelope. But, for reasons which will presently be stated, we may in the mean time, till further light is cast upon this matter, take —239° F. as probably not far from what would be the temperature of the earth's surface were the sun extinguished.

Suppose, now, that we take the mean annual temperature of the Atlantic at, say, 56°.‡ Then 239°+56°=295° represents the number of degrees of rise due to the heat which it receives. In other words, it takes all the heat that the Atlantic receives to maintain its temperature 295° above the temperature of space. Stop the Gulf-stream, and the Atlantic would be deprived of one-fifth of the heat which it possesses. Then, if it takes five parts of heat to maintain a temperature of 295° above that of space, the four parts which would remain after the stream was stopped would only be able to maintain a temperature of four-fifths of 295° or 236° above that of space: the stoppage of the Gulf-stream would therefore deprive the Atlantic of an amount of heat which would be sufficient to maintain its temperature 59° above what it would otherwise be, did it depend alone upon the heat received directly from the sun. It does not, of course, follow that the Gulf-stream actually maintains the temperature 59° above what it would otherwise be were there no ocean-currents, because the actual heating-effect of the stream is neutralized to a very considerable extent by cold currents from the arctic regions. But 59° of rise represent its actual power; consequently 59°, minus the lowering effect of the cold currents, represent the actual rise. What the rise may amount to at any particular place must be determined by other means. * * *

* 'Meteorology,' Section 36.

† Comptes Rendus, July 9, 1838. Taylor's Scientific Memoirs. vol. ix, p. 44, (1846).

‡ The mean temperature of the Atlantic between the tropics and the arctic circle, according to Admiral FitzRoy's chart, is about 60°. But he assigns far too high a temperature for latitudes above 50°. It is probable that 56° is not far from the truth.

At present there is a difference merely of 80° between the mean temperature of the equator and the poles;* but were each part of the globe's surface to depend alone upon the direct heat which it receives from the sun, there ought, according to theory, to be a difference of more than 200°. The annual quantity of heat received at the equator to that received at the poles, supposing the proportionate quantity absorbed by the atmosphere to be the same in both cases, is as 12 to 4·98, or, say, as 12 to 5. Consequently if the temperatures of the equator and the poles be taken as proportionate to the absolute amount of heat received from the sun, then the temperature of the equator above that of space must be to that of the poles above that of space as 12 to 5. What ought, therefore, to be the temperatures of the equator and the poles, did each place depend solely upon the heat which it receives directly from the sun? Were all ocean and aërial currents stopped, so that there could be no transference of heat from one part of the earth's surface to the other, what ought to be the temperatures of the equator and the poles? We can at least arrive at a rough estimate on this point. If we diminish the quantity of warm water conveyed from the equatorial regions to the temperate and arctic regions, the temperature of the equator will begin to rise and the temperature of the poles to sink. It is probable, however, that this process would affect the temperature of the poles more than it would do that of the equator; for as the warm water flows from the equator to the poles, the area over which it is spread becomes less and less. But as the water from the tropics has to raise the temperature of the temperate regions as well as the polar, the difference of effect at the equator and poles might not, on that account, be so very great. Let us take a rough estimate. Say that, as the temperature of the equator rises one degree, the temperature of the poles sinks about one degree and a half. The mean annual temperature of the globe is about 58°. The mean temperature of the equator is 80°, and that of the poles 0°. Let ocean and aërial currents now begin to cease, the temperature of the equator begins to rise and the temperature of the poles to sink. For every degree that the equator rises the poles sink $1\frac{1}{2}$°; and when the currents are all stopped and each place dependent alone upon the direct rays of the sun, the mean annual temperature of the equator above that of space will be to that of the poles, above that of space, as 12 to 5. When this proportion is reached, the equator will be 374° above that of space, and the poles 156°; for 374 is to 156 as 12 is to 5. The temperature of space we have seen to be —239°, consequently the temperature of the equator will in this case be 135°, reckoned from the zero of the Fahrenheit thermometer, and the poles 83° below zero. The equator would therefore be 55° warmer than at present, and the poles 83° colder. The difference between the temperature of the equator and the poles will in this case amount to 218°.

* The mean temperature of the equator, according to Dove, is 79°·7, and that of the north pole 2°·3. But as there is of course some uncertainty regarding the actual mean temperature of the poles, we may take the difference in round numbers at 80°.

Now, if we take into account the quantity of positive energy in the form of heat carried by warm currents from the equator to the temperate and polar regions, and also the quantity of negative energy (cold) carried by cold currents from the polar regions to the equator, we shall find that they are sufficient to reduce the difference of temperature between the poles and the equator from 218° to 80°.

The quantity of heat received in the latitude of London, for example, to that received at the equator is about as 12 to 8. This, according to theory, should produce a difference of about 125°. The temperature of the equator above that of space, as we have seen, would be 374°. Therefore 249° above that of space would represent the temperature of the latitude of London. This would give 10° as its temperature. The stoppage of all ocean and aërial currents would thus increase the difference between the equator and the latitude of London by about 85°. The stoppage of ocean-currents would not be nearly so much felt, of course, in the latitude of London as at the equator and the poles, because, as has been already noticed, in all latitudes midway between the equator and the poles the two sets of currents to a considerable extent compensate each other; viz. the warm currents from the equator raise the temperate, while the cold ones from the poles lower it; but as the warm currents chiefly keep on the surface and the cold return-currents are principally under-currents, the heating effect very greatly exceeds the cooling effect. Now, as we have seen, the stoppage of all currents would raise the temperature of the equator 55°; that is to say, the rise at the equator alone would increase the difference of temperature between the equator and that of London by 55°. But the actual difference, as we have seen, ought to be 85°; consequently the temperature of London would be lowered 30° by the stoppage of the currents. For if we raise the temperature of the equator 55° and lower the temperature of London 30°, we then increase the difference by 85°. The normal temperature of the latitude of London being 40°, the stoppage of all ocean and aërial currents would thus reduce it to 10°. But the Gulf-stream raises the actual mean temperature of London 10° above the normal. Consequently $30°+10°=40°$ represents the actual rise at London due to the influence of the Gulf-stream over and above all the lowering effects resulting from arctic currents. On some parts of the American shores on the latitude of London, the temperature is 10° below the normal. The stoppage of all ocean and aërial currents would therefore lower the temperature there only 20°. * * *

[The author next shows that the climate of the arctic must be affected by the Gulf-stream; and also that the low temperature of the southern hemisphere is owing to the Ocean-currents].

Without Ocean-currents the Globe would not be habitable.—All these foregoing considerations show to what an extent the climatic condition of our globe is due to the thermal influences of ocean-currents.

As regards the northern hemisphere, we have two immense oceans, the Pacific and the Atlantic, extending from the equator to near the north pole, or perhaps to the pole altogether. Between these two oceans lie two great continents, the eastern and the western. Owing to the earth's spherical form, by far too much heat is received at the equator and by far too little at high latitudes to make the earth a suitable habitation for sentient beings. The function of these two great oceans is to remove the heat from the equator and carry it to temperate and polar regions. Aërial currents could not do this. They might remove the heat from the equator, but they could not, as we have already seen, carry it to the temperate and polar regions; for the greater portion of the heat which aërial currents remove from the equator is dissipated into stellar space: the ocean can alone convey the heat to distant shores. But aërial currents have a most important function; for of what avail would it be, though ocean-currents should carry heat to high latitudes, if there were no means of spreading the heat thus conveyed over the land? The function of aërial currents is to do this. Upon this twofold arrangement depends the thermal condition of the globe. Exclude the waters of the Pacific and the Atlantic from temperate and polar regions and place them at the equator, and nothing now existing on the globe could live in high latitudes.

Were these two great oceans placed beside each other on one side of the globe, and the two great continents placed beside each other on the other side, the northern hemisphere would not then be suitable for the present order of things: the land on the central and on the eastern side of the united continent would be by far too cold. * * *

II. GEOLOGY.

1. *Sun-Pictures of Rocky Mountain Scenery;* by Dr. F. V. Hayden, Prof. of Mineralogy and Geology in the University of Pennsylvania.—We have been permitted to make the following extracts from a work of the above title, by Dr. Hayden, which is soon to be published by Julius Bien of New York.* The volume will contain thirty photographic views along the line of the Pacific railroad from Omaha to Sacramento, and besides a description of the geographical and geological features of the country, by Dr. Hayden, whose researches over the Rocky Mountains have often been chronicled in this Journal. We understand that the views selected for publication are generally those that illustrate the geographical and geological features of the region, and are interesting to the student of science as well as to the artist and the lovers of the picturesque in nature.

Communications on the Fossil Plants, Mammals, Fishes, Insects, &c., have been contributed by Messrs. Newberry, Leidy, Cope, and

* The work will be issued to subscribers only, at $25 per copy, and subscriptions should be addressed to Julius Bien, lithographer, 16 and 18 Park Place, New York City.

Scudder, and our extracts are from these papers, and from some of the notes of Dr. Hayden, furnished us by him.

Photograph No. 9, illustrates a peculiar feature of the surface near the junction of Hilter Creek with Green river. The formations are composed of thin layers of fine sand, clay and sandstones or chalky limestones readily disintegrating on the surface, and, as it were, breaking down, so that the water has worn an almost unlimited series of furrows or small ridges with considerable uniformity; sometimes on the summits of the hills or ridges harder portions are left, which wear into castellated forms. This peculiar appearance of the surface is not uncommon all over the west where the Cretaceous and Tertiary formations prevail, especially where they are composed of rather soft clays and sands. The black plastic clays of the Cretaceous beds on the Upper Missouri are subject to this style of weathering, as well as much of the country usually termed the "Bad Lands."

This appearance of the surface carries with it, also, the aspect of desolation. There is little vegetation but the wild sage, Chenopodiaceous shrubs, and other plants which love the alkaline soils. Little depressions occur here and there in which the surface waters accumulate in wet weather; but in the dry season the water evaporates and the surface is left with a thick incrustation of salts of soda, magnesia, &c. I have called the formation along Green river, the Green river shales, from the fact that the sediments are arranged in regular layers, mostly thin like shale, varying, however, in thickness, from that of a knife blade to several feet. This laminated character with the slight variations in color give to the hills the peculiar branded appearance as shown in Photographs 11 and 12 of "Citadel Rock" and "Castle Rock."

Photograph No. 10, illustrates a cut along the railroad through thin layers of a sort of cream-colored chalky limestone. Some of the layers are of a dark brown color and so saturated with petroleum that they burn with a good deal of freedom. This cut is usually called the "Burning Rock cut" from the fact that during the progress of the work, the men built a fire by the side of one of the walls, and the rocks ignited, burning for some days, illuminating the labors of the workmen by night, and filling the valley with a dense smoke by day. In the distance may be seen the banks of Green river formed of similar rocks, which are made up for the most part of silica, lime, and some clay, excellent material for the preservation of organic remains. Besides, all the rocks are more or less impregnated with the oily substance which no doubt originated from the vast quantities of animals which existed in this lake, the remains of which are found in the greatest abundance every where. One of these excavations along the railroad bears the name of the "Petrified Fish Cut," on account of the thousands of beautiful and perfect impressions of fishes which are shown on the surface of the thin slabs or layers of rock. Sometimes a dozen or two of these little herring are found on an area of a square foot. Insects, water plants, and a remarkable specimen of a *feather of a bird* has

been found here. The feather Prof. Marsh regards as a unique specimen, forming a most interesting addition to the bird remains of North America. It is the distal portion of a large feather, with the shaft and vane in such excellent preservation that it may perhaps indicate approximately the nature of the bird to which it belonged.

My collection of *fossil fishes* from these shales was very large, and my success was mostly due to the kind aid of Mr. A. W. Hilliard, a gentleman of intelligence, who superintended the excavations on the line of the railroad, and preserved from time to time such specimens of value as came to his observation.

Prof. E. D. Cope, a distinguished comparative anatomist of Philadelphia, has kindly prepared the following *account of the petrefied fish remains* which were submitted to him for examination, especially for this work. Prof. Cope says:

"The fishes placed in my hands for determination by you, consist of four species, viz: one Acanthopterygian, *Asineops squamifrons* Cope, and three Malacopterygians, *Clupea humilis* Leidy, *Clupea pusilla* Cope, *Cyprinodon levatus* Cope. Those named by the writer were not previously known, and the Asineops represents also a genus not before brought to the notice of scientific men. In ordinary language the last mentioned fish is a perch, but in no degree similar to the white and yellow brought to our markets. The nearest resemblance in general structure is to be found in the black bass of the Ohio and Tennessee rivers, but a closer similarity in form exists in the Red Eye or Goggled Eyed perch of western and southern rivers, Ambloplites of Naturalists. Zoologically it is not very nearly related to either, for it combines with some of their characters, others now existing only in marine fishes of other families. It is an aberrant form of the family of Chætodons, which embraces marine fishes only, and which chiefly abound at the present time in the Indian and Pacific Oceans. But the form and proportions of its fins and scales remind one very much of the swamp and the tide-water sun perch of New Jersey, (*Acantharchius pomotis* Baird, *Bryttus* sp. etc.), and suggest a similarity of habit. The teeth are fine and the dentition brush-like as in the fresh water and many marine perch, and its food was probably much like theirs. As far as zoological evidence goes, there is nothing to indicate whether this species belonged to fresh or salt water; its unarmed character constitutes a pecularity much more prevalent among fresh water than marine fishes, while its zoological affinities, so far as known, are altogether with marine forms. In size this fish exceed the red eye, and was less than the black bass, averaging about as the yellow perch.

"The Clupeas are herring of small species, considerably less than the herring of our coasts. One of the blocks contains the remains of two small shoals of the fry, probably of *Clupea humilis* which were caught suddenly by a slide or fall of calcareous mud, and entombed for the observation of future students. They must have been taken unawares, since they lie with their heads all in one direction as they swam in close bodies. One or two may have had

a moment's warning of the catastrophe, as they have turned a little aside, but they are the exceptions. The fry are from one-half to three quarters of an inch long and upwards.

"The herring, or those without teeth, are chiefly marine, but they run into fresh water to deposit their spawn in the spring of the year, and then return to salt waters. The young run down to the sea in autumn and remain there till old enough to spawn. The size of the fry of the Rocky Mountain herring indicates that they had not long left their spawning ground, while the abundance of adults suggests that they were not far from salt water, their native element. To believe then that the locality from which the specimens were taken, was neither far from fresh nor far from salt water, is reasonable, and this points to a tide or brackish inlet or river. Lastly, the species of Cyprinodon inhabit also tide and brackish waters. Most of the species of the family, as well as of the genus, are inhabitants of fresh water, but they generally, especially the Cyprinodons proper, prefer still and muddy localities, and often occur in water really salt. This habitat distinguishes them especially from Cyprinidæ (minnows and suckers) and Pike. The materials which compose the shales indicate quiet water, and not such as is usually selected by herring for spawning in, while the abundance of adult Clupeas indicate the proximity of salt water. This is far from a satisfactory demonstration of the nature of the water which deposited this mass of shales, but it is the best that can be attained with such a meagre representation of species.

"As to geological age, the indications are rather more satisfactory. The genus Clupea ranges from the Upper Eocene upward, being abundant in the slates of Lebanon and Monte Bolca, while Cyprinodon has been found in neither, but first appears in the middle or lower Miocene in Europe. The Asineops resemble very closely, and I believe essentially, the Pygaeus of Agassiz, of Eocene age, from Monte Bolca. The peculiarities presented by the genus found by Dr. Hayden are of such small significance as to lead me to doubt the beds in question being of later than Eocene in age, though the evidence rests chiefly on this single new and peculiar genus.

"The position of these fishes, 7,000 feet above the level of the sea, furnishes another illustration of the extent of the elevations of regions once connected with the ocean, and the comparatively late period of geological time at which, in this case, this elevation took place."

The *fossil Insects* were examined by Mr. Samuel H. Scudder, of Boston, and he has kindly prepared the following short but very interesting note. The insects were found in the same locality with the fishes.

"The fossil insects found by Dr. Hayden in the tertiary shales of Green river, belong to three species, one being an ant, the others flies. The ant is rather poorly preserved and must he examined with great care before its precise character can be determined.

The larger fly, of which we can distinguish almost the whole of the body, though but little of the wings, evidently belonged to

some species of *Syrphidæ*; it is nearly three quarters of an inch in length and seems to have had a bright colored abdomen, banded with black. In their perfect state, all of this family are very fond of flowers, but the typical species are particularly interesting from the peculiar habits of the larvæ; these are footless grubs which feed on plant lice, piercing them one by one, and sucking out their juice; it is more likely, however, that our species belonged to one of the genera whose larvæ live in the water, or about decaying vegetable substances; larvæ, which from their size and general form might well produce such a fly as this, were found abundantly by Professor Denton; they evidently inhabited the water.

The other fly, judging from the neuration of the wings, which is pretty well preserved, seems to belong to the great family of *Muscidæ*, of which the common house fly is an example; it may probably be referred to a section in which they are nearly or quite wanting, and where the larvæ ordinarily feed upon dead animal matter or upon decomposing plants. Belonging to the same group and perhaps already allied, are the species of *Ephydra* which live in salt marshes, and frequent salt pans to such an extent as to be very troublesome; they have been discovered in the saline waters of some Nevadan lakes, and Professor Denton states that dipterous larvæ, probably of a similar kind, are found in great numbers in lakes impregnated with petroleum. It is probable that the shales, in which these remains occur, were deposited in such a lake."

About a mile west of the "Petrified Fish Bed," is a cut along the railroad which passes through a moderate thickness of buff, chalky limestone filled with impressions of leaves of deciduous trees. These rocks hold a position about one hundred feet above the petroleum shales which contain the fish remains, and therefore the date of their existence may be regarded as somewhat subsequent, though belonging to the same basin. Prof. J. S. Newberry has given these plants a partial examination, and communicated the following interesting notes in the form of a letter.

"I have examined the plants from the Green River beds with as much care as the limited time at my command would permit, and am surprised in not finding among them a single species contained in any of your other great collections at the far west. They, thus far, afford no certain criteria for collating the Green River Tertiaries with those of other localities where you have studied them. The plants from the rocks enclosing the coal at Marshall's mine are more significant, as they include species (*Platanus Haydeni*, which is certainly different from *P. aceroides*), such as were found by you at Carbon Station, and at the mouth of the Yellow Stone. Every collection of fossil plants received from the Tertiary of the west brings to light many new species, and the great diversity which they exhibit proves either a number of plant-bearing horizons, or great localization of the species in the Tertiary flora.

"Among your Green River plants, are only some half dozen species, so well preserved as to be capable of satisfactory identifi-

cation or comparison, but they form a very interesting group. Among them I find two palms, both quite unlike anything before found on this continent. One is a new *Phenicites*, resembling Heer's *Manicaria formosa*. The other but an imperfect fragment, is yet altogether new and strange to me. The most abundant species contained in the collection is a *Magnolia* allied to *M. terminervis* Lesqx., but more elongate and acute—also an oak resembling *Quercus Saffordi* Lesqx. There is another oak in the collection, a laurel (probably), fragments of two ferns too imperfect for determination. On the whole, these plants resemble most those described by Lesquereux from Mississippi, and I am inclined to suspect that they are of the same age. This would make the Green River beds older than you have thought them, and I should want more material before venturing anything more than a suggestion to that effect. I trust you will be able to make other collections from these plant-beds during the present season.

"The specimens contained in the bluff marly limestones of the Green River series are generally not well preserved, and yet I think careful search at the locality where these plants sent me were obtained would result in the discovery of some fine things. I would especially urge a search for fruits.

"The aspect of the small group of plants now before me from Green River, is more tropical than any you have brought from the west, and, as we have reason to believe that our Eocene climate was warmer than the Miocene, and that from the Eocene epoch to the Glacial period a progressive depression of temperature took place, the Green River beds would seem to be of early rather than of late Miocene."

2. *On the Graphite of the Laurentian of Canada;* by J. W. Dawson. (Jour. Geol. Soc., 1869, 112).—In this paper Dr. Dawson sustains the view that the graphite of the Canada Laurentian is of organic origin, and shows that the amount of "graphite in the Lower Laurentian Series is enormous." A limestone in the township of Buckingham on the Ottawa, which is 600 feet or more thick, with some three intercalated bands of gneiss, is in some parts *one-fourth* graphite, and the whole is not less than 20 or 30 per cent graphite. In the adjoining township of Lochaber, a band of limestone 25 to 30 feet thick is so reticulated with graphite, that it is mined for it; and another bed in the same district, 10 to 12 feet thick, yielding 20 per cent of the pure material, is worked. It occurs in equal abundance at other horizons through beds of limestone which have, according to Logan, an aggregate thickness of 3500 feet. In view of the facts Dr. Dawson adds "it is scarcely an exaggeration to maintain that the quantity of carbon in the Laurentian is equal to that in similar areas of the Carboniferous system."

On the mode of occurrence and origin the author observes:

"The beds of graphite near St. John, some of those in the gneiss at Ticonderoga in New York, and at Lochaber and Buckingham and elsewhere in Canada are so pure and regular that one might

fairly compare them with the graphitic coal of Rhode Island. These instances, however, are exceptional, and the greater part of the disseminated and vein graphite might rather be compared in its mode of occurrence to the bituminous matter in bituminous shales and limestones. We may compare the disseminated graphite to that which we find in those districts of Canada in which Silurian and Devonian bituminous shales and limestones have been metamorphosed and converted into graphitic rocks not dissimilar to those in the less altered portions of the Laurentian.* In like manner it seems probable that the numerous reticulating veins of graphite may have been formed by the segregation of bituminous matter into fissures and planes of least resistance, in the manner in which such veins occur in modern bituminous limestones and shales. Such bituminous veins occur in the Lower Carboniferous limestone and shale of Dorchester and Hillsborough, New Brunswich, with an arrangement very similar to that of the veins of graphite; and in the Quebec rocks of Point Levi, veins attaining to a thickness of more than a foot are filled with a coaly matter having a transverse columnar structure and regarded by Logan and Hunt as an altered bitumen. These paleozoic analogies would lead us to infer that the larger part of the Laurentian graphite falls under the second class of deposits above mentioned, and that, if of vegetable origin, the organic matter must have been thoroughly disintegrated and bituminized before it was changed into graphite. This would also give a probability that the vegetation implied was aquatic, or at least that it was accumulated under water.

"Dr. Hunt has, however, observed an indication of terrestrial vegetation, or at least subaërial decay, in the great beds of Laurentian iron-ore. These, if formed in the same manner as more modern deposits of this kind, would imply the reducing and solvent action of substances produced in the decay of plants. In this case such great ore beds as that of Hull, on the Ottawa, 70 feet thick, or that near Newborough, 200 feet thick,† must represent a corresponding quantity of vegetable matter which has totally disappeared. It may be added that similar demands on vegetable matter as a deoxydizing agent are made by the beds and veins of metallic sulphids of the Laurentian, though some of the latter are no doubt of later date than the Laurentian rocks themselves."

He concludes as follows:

"We may sum up these facts and considerations in the following statements:—First, that somewhat obscure traces of organic structure can be detected in the Laurentian graphite; secondly, that the general arrangement and microscopic structure of the substance corresponds with that of the carbonaceous and bituminous matters in marine formations of more modern date; thirdly, that if the Laurentian graphite has been derived from vegetable matter,

* Granby, Melbourne, Owl's Head, &c., 'Geology of Canada,' 1863, p. 599.
† Geology of Canada, 1863.

it has only undergone a metamorphosis similar in kind to that which organic matter in metamorphosed sediment of later age has experienced; fourthly, that the association of the graphitic matter with organic limestone, beds of iron ore, and metallic sulphids greatly strengthens the probability of its vegetable origin; fifthly, that when we consider the immense thickness and extent of the Eozoonal and graphitic limestones and iron-ore deposits of the Laurentian, if we admit the organic origin of the limestone and graphite, we must be prepared to believe that the life of that early period, though it may have existed under low forms, was most copiously developed, and that it equalled, perhaps surpassed, in its results, in the way of geological accumulation, that of any subsequent period.

"In conclusion, this subject opens up several interesting fields of chemical, physiological and geological inquiry. One of these relates to the conclusions stated by Dr. Hunt as to the probable existence of a large amount of carbonic acid in the Laurentian atmosphere, and of much carbonate of lime in the seas of that period, and the possible relation of this to the abundance of certain low forms of plants and animals. Another is the comparison, already instituted by Professor Huxley and Dr. Carpenter, between the conditions of the Laurentian and those of the deeper parts of the modern ocean. Another is the possible occurrence of other forms of animal life than *Eozoon* and Annelids, which I have stated in my paper of 1864, after extensive microscopic study of the Laurentian limestones, to be indicated by the occurrence of calcareous fragments, differing in structure from *Eozoon*, but at present of unknown nature. Another is the effort to bridge over, by further discoveries similar to that of the *Eozoon bavaricum* of Gümbel, the gap now existing between the life of the Lower-Laurentian and that of the Primordial Silurian or Cambrian period. It is scarcely too much to say that these inquries open up a new world of thought and investigation, and hold out the hope of bringing us into the presence of the actual origin of organic life on our planet, though this may perhaps be found to have been Prelaurentian. I would here take the opportunity of stating that in proposing the name *Eozoon* for the first fossil of the Laurentian, and in suggesting for the period the name 'Eozoic,' I have by no means desired to exclude the possibility of forms of life which may have been precursors of what is now to us the dawn of organic existence. Should remains of still older organisms be found in those rocks now known to us only by pebbles in the Laurentian, these names will at least serve to mark an important stage in geological investigation.

3. *On Laurentian Rocks in Nova Scotia;* by T. Sterry Hunt, F.R.S.—The American Journal of Science for May, 1870, contains a paper by Prof. H. Y. Hind on the Geology of Nova Scotia, in which it is said that Dr. Honeyman, in the autumn of 1868, discovered in the Arisaig district, "syenites, diorites, and crystalline limestones, with serpentine, which he then supposed to be of Lau-

rentian age, as he informed me subsequently to the publication of my preliminary report on the Nova Scotia Laurentian. Specimens were sent to Montreal for examination, and instructions were given by Dr. Hunt, who also shared Dr. Honeyman's opinion, to the lapidary, to prepare sections of the serpentinous rock for microscopic examination. By some mischance this was neglected, and the specimens remained unexamined, and indeed forgotten until quite recently, as Dr. Hunt informs me, under date of Feb. 3, 1870. When submitted to the microscopic test the *Eozoon Canadense* was distinctly seen, and Dr. Dawson has confirmed the observations." Prof. Hind further says that the discovery of this fossil "enables geologists to recognize the truth of Dr. Honeyman's opinions, although, by accident, these opinions were not made known or confirmed until after the publication of my report." He again refers to "the just though tardy recognition of the correctness of Dr. Honeyman's views with reference to the age of the limestones and diorites of Arisaig." Pages 354, 355.

Prof. Hind has been deceived in this whole matter, and as he is now, and has been for some months absent in England, I feel called upon, in the interest of truth, to state the facts in the case. In the spring of 1869, Dr. Honeyman, previously employed as an explorer by the Geological Survey of Canada, showed me, in Montreal, a series of specimens collected by him the autumn previous, in the Arisaig district, and including besides syenites and diorites, crystalline limestones, sometimes mixed with a pale green serpentine. These were at once noticed by Mr. Murray of the Geological Survey of Newfoundland, who was present, and myself, as having a close lithological resemblance to the Laurentian rocks, and we mentioned the fact to Dr. Honeyman; while I at once suggested to the lapidary of the Survey, who was in the room, that they were so like the Eozoon-limestones of the Ottawa that it would be well to prepare slices for examination. Meanwhile Dr. Honeyman never made to myself, Sir William Logan, or Dr. Dawson, any suggestions as to the geological age or relations of the rocks in question, and in his official report, handed to Sir William some days later, not only neglects to mention the name of Laurentian, but forgets to make any allusion whatever to the diorites, limestones and serpentines in question. Farther, in summing up his report, he concludes that the district examined by him includes all the rocks between the coal-measures and the gold-bearing slates, thus, by implication, excluding anything lower in the geological series.

I see no reason to believe that the name of Laurentian, first applied by me to these rocks from Arisaig, in Dr. Honeyman's presence, conveyed to his mind, at the time, any notion of geological age, position, or succession, or that he attached any importance to the specimens in question, except as ornamental stones, until the appearance of Prof. Hind's first notice called his attention to the meaning of the term, and to the published descriptions of Laurentian rocks. Were it otherwise, his total silence on the subject

from the autumn of 1868 to some time last winter, is simply incomprehensible.

To Prof. Hind belongs the credit of having declared that the old granitoid rocks of the region are clearly stratified gneisses, and at the base of the series in that region; as they are well known to be on the north side of the Bay of Fundy, where they have been described as Laurentian, and examined by Dr. Dawson, Matthew and Bailey. I had, however, sought in vain for Eozoon in the serpentine-limestones of New Brunswick, but on the receipt of Prof. Hind's preliminary notice, recalled the Arisaig specimens, and recognized in them a form of Eozoon, which, however, according to Dr. Dawson, is specifically distinct from *Eozoon Canadense.*

Montreal, June 7, 1870.

4. *The Ornithosauria: an elementary study of the bones of Pterodactyles;* by H. G. SEELEY of St. John's College, Cambridge: with twelve plates. 132 pp. 8vo, 1870. (Deighton, Bell & Co., Cambridge; Bell & Dalby, London). *Index to the Fossil Remains of Aves, Ornithosauria, and Reptilia, from the Secondary system of strata, arranged in the Woodwardian Museum of the University of Cambridge.* By H. G. SEELEY, of St. John's College, Cambridge; with a prefatory notice by the Rev. ADAM SEDGWICK, LL.D., F.R.S., Woodwardian Professor and Senior Fellow of Trinity College.—The author of these works, the assistant of Prof. Sedgwick at Cambridge, states in his preface that they are portions of the Catalogue of the Woodwardian Museum. This Museum, through the labors mainly of Prof. Sedgwick, is rich in remains of Reptilians from the lower Cretaceous, and is unsurpassed in those of the Cambridge upper Greensand, which has afforded large numbers of bones of Ornithosaurs (Pterosaurs). In the first of these works the able author, Mr. Seeley, reviews briefly what had before been done in connection with the subject of Pterosaurs, discusses their classification and relations, gives the details with regard to the various bones in the collections, and describes and names the several species to which they belong. The author finds that the pneumatic perforations in the bones (seen in the lower jaw, the whole vertebral column, the bones of the fore limb, the scapula and coracoid, the femur and tibia, etc.) are situated as in birds, and indicate a similar system of air-circulation from the lungs, and he argues that this implies the existence of a double heart as in birds. He also points out a relation to birds in the form of the brain, this organ having a very large cerebrum, and, as seen from above, a very small cerebellum abutting against it and pressing to either side the optic lobes (instead of having, as in ordinary Reptiles, the cerebellum behind separated from the cerebrum by the part called the optic lobes). Hence he concludes that the Ornithosaurs were hot-blooded, and makes of them a class of Vertebrates distinct from both Reptiles and Birds. The subject is illustrated by twelve plates.

5. *First Annual Report of the Geological Survey of Indiana, made during the year* 1869; by E. T. Cox, State Geologist, assisted by Prof. Frank H. Bradley, Dr. Rufus Hammond, and Dr. G. M. Levette. 240 pp. 8vo. Indianapolis, 1870.—This first annual Report of the Geological Survey of Indiana, under Prof. Cox, consists of a General Report by the head of the survey, and special Reports by Prof. Bradley on Vermillion Co., and Dr. Hammond on Franklin Co. Prof. Cox has begun a good work with reference to the coal formation, and has already proved that the number of coal beds, and the subdivisions of the formation, as laid down by Owen and others in Indiana and Kentucky, are wholly erroneous. As one example he shows that the "Mahoning Sandstone" and the "Anvil Rock Sandstone" are actually the same rock. Prof. Cox states that he is not yet prepared to present his own conclusions, but he shall make it a special object, in the course of the Survey, to ascertain the precise facts on this important subject. Much valuable information is presented by him respecting the coal formation and other rocks and products of Clay county. Prof. Bradley describes with considerable detail the Carboniferous rocks of Vermillion county, mentioning many of their fossils, and giving some account of the beds of iron ore and fire clay which they contain. He says that in this county the "boulder clay" has a depth near Perrysville of about 100 feet before reaching the underlying quicksand; and that in some places it is 125 feet thick. It contains boulders of limestone and of metamorphic rocks, which are sometimes striated, besides occasional rolled masses of galena and native copper. There is much in the volume we might quote, with interest to our readers, if space allowed. The report closes with a list of the Mammals and Birds of Franklin Co. It is illustrated by three maps, and a large plate of sections.

6. *Meek on Crinoids.*—The views of Mr. Meek on the anatomy of of the Paleozoic Crinoids, presented in a paper reprinted from the Proceedings of the Academy of Nat. Sciences of Philadelphia, in vol. xlviii of this Journal (at p. 23), are fully endorsed in a letter written by Prof. Sars of Norway, the profound investigator of species of recent Crinoids, a few days only before his death. Prof. Sars observes, judging from the photographs of the specimens which had reached him, that Mr. Meek's conclusions seem to him to be perfectly founded, and to result with logical necessity from the investigations;" and that "they spread unexpected light upon these curious extinct Crinoids." Letters from Prof. Wyville Thomson, of Belfast, and Dr. Lütken, of Copenhagen, also approve entirely of his conclusions.

7. *The lifted and subsided Rocks of America, with their influences on the oceanic, atmospheric, and land currents and the distribution of Races;* by Geo. Catlin. 228 pp., 12mo. London, 1870. (Trübner & Co.)—The writer of this work, well known for his travels among the American Indians, here treats of mountain drainage, upheavals, metamorphism, making of mountain chains, sinking of mountains, and of the Indian races of America. He

presents his geological views and criticisms with great positiveness, which is consistent with the fact of his limited knowledge of the subject.

8. *Geological Survey of Iowa.*—The State of Iowa has ordered the printing of three thousand copies of the Report of the Geology of the State by Prof. White. The work will consist of two royal octavo volumes, in the style of Hall's Geology of Iowa and of the Illinois Geological Report, and will be well illustrated. We regret to have to add that the Legislature has discontinued the Survey.

III. ZOOLOGY.

1. *Die bis jetzt bekannten Schildkröten, u. d. bei Kelheim u. Hannover neu aufgefunden altesten Arten derselben;* von Dr. G. A. Maack. Cambridge, Mass. (Cassel, 1869,—from H. von Meyer's Palæontographica).—This useful work, a 4to of 146 pp., is contributed by Dr. Maack, whose arrival in the United States and occupation in the Museum of Comparative Zoology, we take pleasure in noticing. It embraces a synopsis of the species of extinct Testudinata, arranged in the order of geological succession, rather than according to structural affinity. The number thus enumerated is 192, of which 26 are assigned to the Testudinidæ, 114 to the Emydidæ, including the Pleurodira; 27 to the Trionychidæ, and 25 to the Cheloniidæ. The species are not nearly all described, but their enumeration forms an invaluable hand-book to the student of the subject. The stratigraphical table given, adds to its value. From it we perceive that the Tertiaries embrace the majority of the species, and the Cretaceous and Jurassic periods successively fewer. The upper Jurassic of Switzerland, Bavaria and Hanover have furnished the oldest known Testidinata, unless the Chelytherium of the Würtembergian Trias belong to the order, a point still doubtful. The number of Jurassic species known was 15, to which Dr. Maack adds 3, based on remains mostly from Hanover, from a stratum of prior deposit to those of Switzerland and Bavaria.

Before noticing the types of Cretaceous and Jurassic Tortoises, it must be observed that the system of Strach which Dr. Maack adopts, is a very defective one, and far behind the requirements of modern zoology and paleontology. The structural features defining the suborders and families are overlooked in this. For example, one of the primary divisions of the order, the Pleurodira, is included among the Emydidæ, whereas it embraces a series of families distinguished by features quite similar to those defining the remaining families from each other.* In consequence several conclusions are reached which require modification. The genus *Platemys* as adopted may be cited. It embraces 9 species according to the present work, the genus Pleurosternum of Owen being referred to it. This is done because the additional pair of thoracic bones which characterizes it is found in a rudimental

* See Proc. Acad. Nat. Sci., 1868, p. 282.

condition in *Platemys Bowerbankii*, and *Platemys Bullockii* of Owen presents the intermarginal scuta of *Pleurosternum*, and because of the general resemblance in specific characters between the latter and the *Pl. concinnum*. To us, however, the genus *Pleurosternum* appears to be Cryptodire not Pleurodire, as it lacks the integular scutum of the latter suborder, and to represent a peculiar family of that group characterized by the possession of ten instead of eight sternal bones. *Platemys Bullockii*, *P. Bowerbankii* and *Emys lævis* Ow. and Bell appear on the other hand to be Pleurodira, and to be referable to two families of that suborder. The *Pl. Bullockii*, on account of its five pairs of sternal bones, to the Sternothæridæ, and on account of its intermarginal scuta, to a new genus which I have called *Digerrhum*. The last two, in their rudimental fifth pair of sternals resemble many Pleurodira, and cannot be distinguished from the genus *Podocnemis* now living in the Amazon. The *P. sulcatus* Leidy is near to Podocnemis also, but represents a distinct genus, characteristic of the Cretaceous, which I call *Taphrosphys;* there are six species in North America. After these deductions the only Platemydes that remain are *P. Mantellii* and *P. Dixonii* of Owen.

The new forms described by Maack are of much interest. His *Chelonides Wittei* is one of the group found in both Jurassic and Cretaceous strata in Europe and North America, which combines *Chelydroid* and *Chelonioid* characters so as to render it difficult to be assured as to which group they truly represent. The characters of the carapace in most, and of the plastron in many, are those of the latter, while those of the limbs, the crucial test in this case, are those of the former. Two of the North American genera add one or two costal bones, a character of importance and one not hitherto met with in the order; these may be regarded as the type of a peculiar family with the name of the *Propleuridæ*, including the genera *Osteopygis* and *Propleura*. The family with eight costals includes *Chelonides* Maack, which seems to be near *Chelonemys* Jourd, as he has placed it,—with *Platychelys*, *Hydropelta*, *Idiochelys* and some other European forms which, with *Catapleura* and *Lytoloma* from North America, are nearer *Chelydra*, and I cannot at present find characters which distinguish them as a family from the existing forms. In *Stylemys* Maack, the second new genus introduced into the present work, the sternum is without fontanelles, and resembles entirely that of *Adocus* from the American Cretaceous, while the carapace and femur are of the type of *Osteopygis*. Until further investigated it should remain as an *Emydoid*, as placed by Dr. Maack. Two species are described, *S. Lindenensis* and *S. Hannoveranus*. The use of the name *Stylemys* is a *faux pas*, since it must probably be used for a genus of *Emydidæ* described by Leidy from the Miocene and Pliocene of Nebraska and Dakota. True, it was originally established on untenable characters, and reunited by its proposer with *Testudo*. But I have been able to point out (Trans. Amer. Phil. Soc., 1869, 123) that the species so originally named are really *Emydidæ*

having Testudinine characters, and requiring a distinct generic name, for which *Stylemys* should perhaps be adopted. Dr. Maack will therefore be necessitated to find another for that which he describes.

This and other rectifications relating to North American species not having been published prior to Dr. Maack's work, he was unable to take advantage of them. I therefore append the following list as supplementary to it.

POSTPLIOCENE SPECIES.

Emydidæ.

Cistudo eurypygia Cope. *Emys petrolei* Leidy.

PLIOCENE AND MIOCENE SPECIES.

Emydidæ.

Stylemys Nebrascensis Leidy. *S. Niobrarensis* Leidy.

Trionychidæ.

Trionyx lima Cope. *T. Buiei* Cope.

Cheloniidæ.

Puppigerus grandævus Leidy.

EOCENE SPECIES.

Trionychidæ.

? Trionyx pennatus Cope.

? Cheloniidæ.

Lembonax polemicus Cope. *Puppigerus parviscutatus* Cope.

CRETACEOUS SPECIES.

Emydidæ.

Emys turgidus Cope.
E. petrosus Cope.
E. firmus Leidy.
E. Stevensoni Leidy.
Adocus beatus Leidy.
Adocus agilis Cope.
A. pravus Leidy.
A. Vyomingensis Leidy.
A. pectoralis Cope. (*Pleurosternum* olim.)

Chelydridæ.

Lytoloma Jeanesii Cope.
L. angusta Cope.
Catapleura repanda Cope.
Pentresius ornatus Leidy.

Propleuridæ.

Euclastes platyops Cope.
Propleura sopita Leidy.
Osteopygis emarginatus Cope.
Osteopygis platylomus Cope.
O. chelydrinus Cope.

Trionychidæ.

Trionyx halophilus Cope.
T. priscus Leidy.
Trionyx guttatus Leidy.

Podocnemididæ.

Bothremys Cookii Leidy.
Taphrosphys molops Cope.
T. sulcatus Leidy.
T. longinuchus Cope.
Taphrosphys Leslianus Cope.
T. strenuus Cope.
T. nodosus Cope.

Thirty-eight species. *Puppigerus* embraces all of the Chelones described by Owen and Bell from Sheppey. To *Lytoloma* perhaps belong the two Chelones described and figured by Faujâs and Cuvier from the Cretaceous of Maestricht named *Ch. cretacea* by Keferstein and *Ch. Faujasii* by Maack. E. D. C.

2. *On Discosaurus and its allies;* by Dr. J. LEIDY. (Proc. Acad. Nat. Sci. Philad., 1870, p. 18).—On the 5th of April, Dr. Leidy made remarks on Discosaurus and its allies, additional to those published at p. 392 of the last volume of this Journal; and in the course of it discusses the relations of *Cimoliasaurus*, as originally established, to *Discosaurus*. On this point he observes:

"In the first place, by comparison with the skeleton of the Kansas saurian, we observe that the position in the column assigned to the vertebral bodies of *Cimoliasaurus* was incorrect, and this probably contributed to mislead Prof. Cope in his examination of the skeleton of the Kansas saurian.

"The vertebral specimens referred to *Cimoliasaurus* consisted of two sets of specimens, from two different individuals, both from the greensand of Burlington Co., N. J. They are described in 'Cretaceous Reptiles,' page 25, and characteristic ones presented in plates v and vi.

"The eleven vertebræ considered as lumbar, and represented by figs. 17–19, pl. v, and 16–18, pl. vi, are evidently cervicals. Those considered as dorsals on page 26, and represented in figs. 13–16, pl. v, are at least in part posterior cervicals. Of the fourteen vertebræ referred to on page 27 as dorsals and lumbars, those described and represented in figs. 1–5, pl. vi, are alone dorsals, while the others described and represented in figs. 6–9 are posterior, and those of figs. 10–18 more anterior cervicals.

"The cervicals of *Cimoliasaurus* are so different in their proportions from those of the Kansas saurian that there can be no question as to the distinction of the two animals, at least as species.

"Do all the remains originally referred to *Discosaurus* belong to this genus as distinct from *Cimoliasaurus?* I suspect that those from New Jersey belong to the latter. The animals indicated by all the fossils which have been under consideration are Plesiosauroid, and, as in recognized species of *Plesiosaurus*, there is much variability in the number, proportions, and other characters of the cervicals without a corresponding extent of variation in other parts of the vertebral column, we would be prepared to find in *Cimoliasaurus* nearly the same kind of caudals as in *Discosaurus*.

"Prof. Cope, in his 'Synopsis of the Extinct Batrachia and Reptilia,' pt. i, 1869, p. 56, describes two vertebral specimens from the lower bed of the Cretaceous green sand of Gloucester and Monmouth counties, which he attributes to a species with the name of *Elasmosaurus orientalis*. The specimens described as caudals are seen, by comparison with the Kansas skeleton, to be cervicals."

The species referred to in this paper—all Cretaceous—are finally as follows: 1. *Discosaurus vetustus* Leidy (Cimoliasaurus magnus, and C. vetustus, of *Cope*), from Alabama. 2. *Disc. grandis* (Bri-

mosaurus grandis *Leidy*, Cim. grandis *Cope*), from Arkansas. 3. *Disc. carinatus* (Elasm. platyurus, and Disc. carinatus of *Cope*) from Kansas. 4. *Disc. magnus* (Cim. magnus *Leidy*, Disc. vetustus in part? *Leidy*), from New Jersey. 5. *Disc. planior* (Disc. vetustus in part, *Leidy*), from Mississippi. 6. *Disc. orientalis* (Elasm. orientalis *Cope*), from New Jersey.

3. *On Elasmosaurus platyurus* Cope; by Prof. E. D. COPE. (Communicated by the author).—I observe, in the last number of this Journal, that Prof. J. Leidy criticizes my determination of the structural characters and generic relationships of the above reptile, stating that I have reversed the direction of the vertebral column, describing the cervical as the caudal series and vice versa, and that it is the same as his previously described genus *Discosaurus*. On these points I would make the following observations.

First, as to the direction of the vertebral column, I have little doubt that Prof. Leidy is correct in his determination, especially since I have already pointed out, that, assuming the direction I gave it to be true, the vertebral articulations, and the scapular and pelvic arches, appeared to be the reverse of those of Plesiosaurus. Prof. Leidy does not, however, allude to the principal cause of this error, which was the similar reversal of the vertebral column in his descriptions of his genus *Cimoliasaurus*, first published in 1851, and re-published with 2 plates in 1864. Having my mind pre-occupied with this determination and not suspecting the error, I arranged *Elasmosaurus* in accordance with it. The great size of the clavicle, and lack of special characters of the scapular arch, as mesosternal, etc., and consequent close resemblance to many reptilian fishes, rendered the error more easy, while the coincident discovery of several reptilian forms with zygosphene articulation, attracted my atention to that character.

It might be added that the description and restoration are correct in the "Synopsis Extinct Batr. Reptilia, etc., North America," the error having appeared in a few extra copies only. Also that the accounts already given will require scarcely any modification, the caudal region like the cervical being very long, and less depressed than in *Cimoliasaurus*, etc.

Second, from the identification of *Elasmosaurus* with *Discosaurus*, I entirely dissent. Dr. Leidy, having assumed the cervicals of *Cimoliasaurus* to be lumbars, and stating it as "probable that part of the series described as lumbars may be regarded as representing sacrals and caudals,"* referred the true caudals of the same genus, to another supposed genus, under the name of *Discosaurus*. Anterior caudals of *Cimoliasaurus magnus* he regarded as cervicals of the new genus. But, entertaining a suspicion that the two genera might be one, he says that in this case, they "represent cervicals, dorsals and lumbars of *Discosaurus*," (i. e. *Cimoliasaurus*, the name earliest given). Having shown the identity of the two forms in accordance with the structure of *Elasmosaurus*, I failed to reverse the arrangement adopted by Prof. Leidy. Had I

* Cretaceous Reptiles, N. America, 29.

done so, *Cimoliasaurus* and *Discosaurus* would have become synonyms of *Plesiosaurus*, since no characters are known by which to distinguish them. As it is, I preserve the first, on the supposition that its scapular arch, will be found to present the peculiarities belonging to *Elasmosaurus*.

There are, however, several species included in Leidy's last description of *Discosaurus vetustus*, as he suggests, but believing that "the material was not sufficient to justify a separation," he allowed them to remain together. A portion of this material from New Jersey belongs undoubtedly to *Cimoliasaurus magnus*, and the other specimens (two vertebræ), which present a few peculiarities, are recorded in my Synopsis Extinct Batrachia, Reptilia, etc., as *Cimoliasaurus vetustus*. I presume that it is on these two vertebræ that Leidy bases his reference of *Elasmosaurus* to *Discosaurus*. If the evidence furnished by these was "insufficient to justify their separation" from *C. magnus*, it is certainly insufficient to justify their reference to another genus. The proximal caudals of *Elasmosaurus* and *Cimoliasaurus* are identical, but the median and distal caudals of the two are quite distinct. In *E. platyurus* they present a deep median groove beneath and a rib-like elevation on each side. No such vertebræ have been described as referable to *Cimoliasaurus*, and there is no evidence to prove that the slightly angulate caudals among those referred to *C. vetustus* by Prof. Leidy did not belong to the medial caudal region of a *Cimoliasaurus*.* In a little notice furnished to LeConte's report on the Geology of the Union Pacific R. R., southern division, written almost as soon as I received the fossil, I temporarily referred the caudals to *Discosaurus*, not being generally willing to establish a new genus on caudal vetebræ or other distal portions.

In conclusion, it may be summarily stated that: 1, *Discosaurus* was erroneously constituted; 2, that characters separating it from *Plesiosaurus* were not adduced; 3, that it was not distinguished from *Cimoliasaurus;* 4, that *Discosaurus vetustus* embraces at least two species, one of which is *Cimoliasaurus magnus ;* and, 5, the other cannot be proven to be an *Elasmosaurus*, but scarcely differs from corresponding parts of *Cimoliasaurus magnus*.

4. *A new species of Tapir, from Guatemala ;* by Prof. THEODORE GILL. (Extract from a letter to one of the Editors).—The Smithsonian Institution has now the skulls of four adults and one young of the Tapir of Guatemala, and strange as it may appear, they seem conclusively to prove that the Tapir of that region is a different species from that of Panama, belonging, however, to the same genus—*Elasmognathus*. The most obvious differences are in the development of the nasal and frontal bones, but those are confirmed by the differences in the dentition, especially in the form of the first premolar of each jaw. The nasal bones of the young, compared with those of the corresponding age of *E. Bairdii*,

* It can hardly be doubted that the median and distal caudals of Cimoliasaurus are angulate beneath, to produce terminal planes for the chevrons, in accordance with the structure of Plesiosaurus.

are wider, especially in front of the "pits," and exhibit basilar processes recurrent forward along the frontal bones, like those of *Tapirus*, but less developed, and the grooves for the nasal cartilages are deeper. As the animal advances in age, however, the frontals would appear to grow forward and force apart the nasals, which apparently do not increase, or even diminish in size, and sooner or later are fused with the frontals! This strange outgrowth of the frontal bones has been verified on four adults, and consequently the natural suggestion, that it was a monstrous individual feature, is rendered improbable. The first premolars of the Guatemalan form, in comparison with those of *E. Bairdii*, are in the upper jaw much narrower and divided into halves, the anterior of which is compressed and of almost uniform width, while the inner face of the posterior half bulges abruptly inward; in the lower jaw, they are also narrower, and the anterior cusp is separated by a vertical groove on the inner face of the tooth. All the specimens were obtained by Capt. John M. Dow, who "was told by the party who gave [him] the skulls that the young *are not* marked on the body with longitudinal light colored stripes, like *E. Bairdii*," and he believes that "this want of marking is evidently constant in the young of the species found in Guatemala." This, if confirmed by Capt. Dow himself from autopsy, would furnish a remarkable exception in the family; but without attaching undue importance to the statement, the differences already enumerated seem sufficient to establish its specific rank, and I shall describe it at length under the name *Elasmognathus Dowii*. I may add that Prof. Baird from the first has insisted on its distinctness.

5. *Contributions to the Theory of Natural Selection: A Series of Essays;* by ALFRED RUSSELL WALLACE, author of the Malay Archipelago, etc. 384 pp. 8vo. London, 1870. (Macmillan & Co).—Mr. Wallace, the author of this work, has the credit of being an independent originator of the doctrine of "Natural Selection." His first memoir, published in 1855, led to the publication by Darwin of his first work (in 1859) on the Origin of Species, as Darwin states in his preface. Darwin's views had been partly in manuscript for more than ten years, and had been made known to Lyell in 1844. Wallace's work will be read with interest by all who would acquaint themselves with the arguments and facts upon which the hypothesis of Natural Selection rests, and also for its original and curious observations on the geographical distribution and habits of some animals. The author differs widely on some points from Darwin and Huxley. He has a chapter, evincing profound and earnest thought, on the Limits of Natural Selection as applied to Man, in which he shows that Man could not have been made through Natural Selection, appealing for proof to the size of his brain; his naked and soft skin; his feet and hands; man's mental faculties, and his moral sense. He speaks of man as an ever-advancing, spiritual being, the ultimate aim of all organized existence; of his will, as not a product of nature's chemistry, but above

nature; and he says that the inference which he draws from the facts reviewed is that "a superior intelligence has guided the development of man in a definite direction, and for a special purpose, just as man guides the development of many animal and vegetable forms." He also argues that "all force is probably will-force;" and thus, that "the whole universe is not merely dependent on, but actually *is*, the WILL of higher intelligences, or of one Supreme Intelligence." We cite a few paragraphs from the part of his argument based on a comparison of the brains of Man and the Man-apes.

"The collections of Dr. J. B. Davis and Dr. Morton give the following as the average internal capacity of the cranium in the chief races:—Teutonic family, 94 cubic inches; Esquimaux, 91 cubic inches; Negroes, 85 cubic inches; Australians and Tasmanians, 82 cubic inches; Bushmen, 77 cubic inches. These last numbers, however, are deduced from comparatively few specimens, and may be below the average, just as a small number of Finns and Cossacks give 98 cubic inches, or considerably more than that of the German races. It is evident, therefore, that the absolute bulk of the brain is not necessarily much less in savage than in civilized man, for Esquimaux skulls are known with a capacity of 113 inches, or hardly less than the largest among Europeans.* But what is still more extraordinary, the few remains yet known of pre-historic man do not indicate any material diminution in the size of the brain case. A Swiss skull of the stone age, found in the lake dwelling of Meilen, corresponded exactly to that of a Swiss youth of the present day. The celebrated Neanderthal skull had a larger circumference than the average, and its capacity, indicating actual mass of brain, is estimated to have been not less than 75 cubic inches, or nearly the average of existing Australian crania. The Engis skull, perhaps the oldest known, and which, according to Sir John Lubbock, "there seems no doubt was really contemporary with the mammoth and the cave bear," is yet according to Professor Huxley, "a fair average skull, which might have belonged to a philosopher, or might have contained the thoughtless brains of a savage." Of the cave men of Les Eyzies, who were undoubtedly contemporary with the reindeer in the South of France, Professor Paul Broca says (in a paper read before the Congress of Pre-historic Archæology in 1868)—"The great capacity of the brain, the development of the frontal region, the fine elliptical form of the anterior part of the profile of the skull, are incontestible characteristics of superiority, such as we are accustomed to meet with in civilized races." * * * We cannot fail to be struck with the apparant anomaly, that many of the lowest savages should have as much brains as average Europeans. The idea is suggested of a surplusage of power; of an instrument beyond the need of its possessor. In order to discover if there is any foundation for this notion, let us compare the brain of man with that of animals. The adult male

* While the largest Teutonic skull in Dr. Davis's collection is 112·4 cubic inches, there is an Araucanian of 115·5, an Esquimaux of 113·1, a Marquesan of 110·6, a Negro of 105·8, and even an Australian of 104·5 cubic inches.

Orang-utan is quite as bulky as a small sized man, while the Gorilla is considerably above the average size of man, as estimated by bulk and weight; yet the former has a brain of only 28 cubic inches, the latter, one of 30, or, in the largest specimen yet known, of 34½ cubic inches. * * *

"We see, then, that whether we compare the savage with the higher developments of man, or with the brutes around him, we are alike driven to the conclusion that in his large and well-developed brain he possesses an organ quite disproportionate to his actual requirements—an organ that seems prepared in advance, only to be fully utilized as he progresses in civilization. A brain slightly larger than that of the gorilla would, according to the evidence before us, fully have sufficed for the limited mental development of the savage; and we must therefore admit, that the large brain he actually possesses could never have been solely developed by any of those laws of evolution, whose essence is, that they lead to a degree of organization exactly proportionate to the wants of each species, never beyond those wants—that no preparation can be made for the future development of the race—that one part of the body can never increase in size or complexity, except in strict co-ordination to the pressing wants of the whole. The brain of pre-historic and of savage man seems to me to prove the existence of some power, distinct from that which has guided the development of the lower animals through their ever-varying forms of being."

IV. ASTRONOMY.

1. *Cordova Observatory under the direction of* Dr. B. A. Gould. —Dr. Gould has, at our request, furnished us with the following statement with regard to the Cordova Observatory which has been placed under his charge. He sailed with his family for South America in the latter part of May.

The Argentine Congress voted to establish a national observatory at Cordova, at the instance of President Sarmiento, and through the exertions of the present Minister of Public Instruction, Dr. Avelleneda,—who invited me to organize and take charge of it, knowing my desire to extend the catalogue of the southern heavens beyond the limit of 30° to which the zones of Argelander extend. Bessel went through the region from 45° N. to 15° S. with systematic zone-observations at Königsberg, which have since been reduced and published in two catalogues by Weisse of Cracow. Argelander carried the same systematic scrutiny with the Meridian Circle, from Bessel's Northern limit to the pole, and afterwards from Bessel's southern limit to 30° S.

Since then Gilliss has observed a series of zones for 30° around the south pole; but the reduction of these, although very far advanced, was not completed at the time of his death, and the Ms. is now stored somewhere in Washington. Let us hope that it may at some time be recovered, the work completed and given to the world.

My hope and aim is to begin a few degrees North of Argelander's southern limit, say at 26° or 27°, and to carry southward a system of zone observations to some declination beyond Gilliss's northern limit, thus rendering comparisons easy with both these other labors, and permitting the easy determination of the corrections needful for reducing positions of any one of the three series to corresponding ones for the other. It is of course impossible to arrange in advance the details of such an undertaking, but my expectation is to go over the region in question in zones 2° wide, (except in the vicinity of the Milky Way where the width would be but one-half as great,) up to a declination of about 55°, after which the width would be gradually increased as the declinations became greater. Within these zones all stars seen as bright as the 9th magnitude would be observed, so far as possible, moving the telescope in altitude when no bright star is in the field until some one becomes visible, according to the well known method of zone-observations.

For reducing the observations, differential methods will probably be employed, inasmuch as the time now assigned for my absence from home would be inadequate for proper discussion of the corrections required for nice determinations of an absolute character. Still it is my present purpose, so far as possible, to make such subsidiary determinations as might hereafter be needed in any attempt at computing the observations absolutely. But as I hardly venture to anticipate any opportunity of making a thorough determination of the constants of refraction, or of the errors of graduation, it seems best to arrange for a differential computation at least at first.

It is improbable that a sufficient number of well determined stars will be found available even for this differential reduction, and the necessity may thus be entailed of determining the comparison-stars myself, this determination, however, itself depending upon standard star places. So far as possible I propose employing those heretofore determined by me, and published by the coast survey, which form the basis of the star places of the American Nautical Almanac.

With these observations of position it is my hope to combine others of a physical character to some extent; but in the presence of a plan implying so much labor and effort, it would be unwise to rely upon the possibility of accomplishing much more than the zone-work.

The Meteorological relations of the place are very peculiar, but I dare not undertake any connected series of observations bearing upon these, without self-registering apparatus, which is beyond my means.

Cordova is one of the oldest cities, and contains the oldest university, of the Western hemisphere. It is situated in 31°½ S. latitude, on the boundary of the Pampa, where the land begins to rise toward the group of mountains known as the Sierra de Cordova. It is connected with Rosario, on the Parana, by the Cen-

tral Argentine railway, which has probably been already opened to travel through its entire length of about 250 miles, although information to that effect has not yet reached this country.

The two largest instruments will be a Repsold meridian-circle of 54 inches focal length and 4½ inches aperture, and an equatorial by Alvan Clark & Sons, provided with the 11 inch object-glass, by Fitz, lately in the posession of W. Rutherfurd who has supplied its place by one of 13 inches. A photometer by Ausfeld of Gotha, according to Zöllner's latest form, has been constructed under the supervision of Prof. Zöllner himself; a spectroscope will be furnished by Merz of Munich, and a clock by Tiede of Berlin.

The Scientific institutions of the U. S. have afforded the expedition every possible assistance. The Coast Survey lends a circuit-breaking clock, a chronograph, and a portable transit; the Smithsonian Institution lends a zenith telescope; the American Academy of Arts and Sciences of Boston [probably] a photometer and spectroscope; the Washington Observatory and the Nautical Almanac have greatly aided the undertaking by gifts of books and by a manuscript copy of Gilliss's catalogue of Standard Stars; and from the astronomers of England, Germany and Russia important assistance has been freely and effectively contributed, in the order and supervision of instruments and apparatus, and by the gift of books, as well as by important and valuable suggestions.

Four assistants will accompany me, Messrs. Miles Rock, John M. Thome, Clarence L. Hathaway and William M. Davis, Jr. We hope to reach Buenos Ayres not later than the middle of August.

The building is now under construction in Boston. The means available proved inadequate for its construction according to the original plan, which was in the form of a cross, with four square rooms about its center, and turrets at its four extremities. One half of it will be first erected; and it is hoped that the remaining portion will speedily be added.

2. *Recent Auroral displays in the United States.*—We are now near the period of a maximum of the solar spots, and if auroral displays observe a similar periodicity, we must also be near an auroral maximum. The reports made by the observers of the Smithsonian Institution as published with the monthly Reports of the Department of Agriculture have been examined to ascertain the number of auroras recorded upon successive years. Only an occasional observation of the aurora appears in these Reports until the year 1869. It is presumed that the Department of Agriculture did not regard the auroral observations of the preceding years as possessing sufficient general interest to authorize their publication. The following are the dates upon which auroras were reported by at least one of the observers of the Smithsonian Institution for the year 1869, and for three months of the year 1870.

1869, Jan.	3, 5, 6, 7, 10, 13, 17, 18, 19, 20, 21, 22, 30,	Total	13 days.
Feb.	2, 3, 4, 5, 6, 8, 9, 10, 11. 15, 17, 18, 19,	"	13 "
March	1, 2, 4, 5, 6, 7. 8, 9, 10, 11, 12, 13, 14, 16, 17, 18, 30, 31,	"	18 "
April	1, 2, 3, 4, 5. 6, 7, 8, 9, 10, 11, 12, 13, 14, 15, 16, 17, 19, 22, 23, 28, 29, 30,	"	28 "

1869, May 3, 4, 5, 6, 7, 8, 9, 10, 12, 13, 24, 27, 28, 29,	Total,	14 days.
June 4, 5, 6, 7, 8, 12, 15, 16, 19, 20, 23, 25, 26, 27, 28, 29, 30,	"	17 "
July 2, 3, 5, 10, 11, 12, 13, 14, 15, 16, 17, 18, 19, 20, 25, 26, 28, 29,	"	18 "
Aug. 1, 3, 5, 6, 7, 10, 14, 15, 16, 22, 24, 27, 28, 29, 30,	"	15 "
Sept. 1, 2, 3, 4, 5, 6, 8, 9, 10, 11, 12, 13, 14, 15, 16, 19, 24, 26, 27, 28, 29, 30,	"	22 "
Oct. 1, 2, 4, 5, 6, 8, 9, 12, 21, 22, 24, 25, 26, 27, 28, 30, 31,	"	17 "
Nov. 1, 6, 8, 9, 10, 12, 25, 26, 29,	"	9 "
Dec. 4, 5, 6, 7, 13, 14, 15, 18, 22, 25, 26, 29, 30,	"	13 "
Total for the year 1869,		192 days.
1870, Jan. 1, 2, 3, 4, 5, 6, 7, 8, 9, 16, 18, 24, 25, 26, 27, 28, 29, 30, 31,	Total	19 days.
Feb. 1, 2, 3, 4, 5, 6, 10, 11, 12, 13, 20, 21, 23, 24, 25, 26, 27, 28,	"	18 "
March 1, 2, 3, 4, 6, 8, 9, 10, 11, 13, 19, 20, 21, 22, 23, 24, 25, 26, 27, 28, 29, 30, 31,	"	23 "
Total for three months of 1870,		60 "

These observations indicate a very remarkable number of auroras, but it should be remembered that they are derived from the reports of 350 observers spread over a territory embracing 56 degrees of longitude and 20 degrees of latitude. In order that we may be able to make a comparison with former years, it is desirable to have a similar summary of the reports of all the Smithsonian observers; and it is to be hoped that the Secretary of that Institution will furnish us with such a summary for each year since the Smithsonian system of meteorological observations was commenced.

The following summary of the auroral observations made for five years at Depauville, N. Y. (lat. 44° 7′ N. long. 76° 3′ W.) has been furnished by Mr. Henry Haas.

	Jan.	Feb.	March.	April.	May.	June.	July.	Aug.	Sept.	Oct.	Nov.	Dec.	Year.
1865	2	3	3	5	1	5	5	5	4	5	2	2	42
1866	1	1	4	3	7	2	3	4	6	5	4	2	42
1867	2	1	4	3	5	2	3	6	10	5	2	1	44
1868	3	3	10	4	6	4	3	3	2	1	0	1	40
1869	4	1	4	6	5	2	4	4	8	1	2	3	44

The number of auroras reported for the different years is remarkably uniform, and similar results have been found at other stations where the annual number of auroras rises as high as fifty or upwards; but in lower latitudes where auroral displays are less frequent, the indications of periodicity are unmistakable.

V. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

1. *Variations in the Eccentricity of the Earth's Orbit.*—In vol. xlvi of this Journal (1868), Prof. J. N. Stockwell has a paper, with a chart, illustrating the variations in the eccentricity of the Earth's orbit for the past one million of years, starting from a point 175,000 years back of the present time; and in an earlier memoir "On the Secular Equation of the Moon's mean motion" published at Cambridge in 1867, he has given a similar chart for the million of years following. His calculations were made for intervals of 10,000 years, and hence they are much more exact than the earlier chart

of Croll, whose intervals were, for the major part of it, 50,000 years. The main point put forward by Prof. Stockwell in his Cambridge memoir has not been accepted by astronomers, but this does not affect his calculations of the eccentricity. According to his results for past time, a low maximum occurred about 100,000 years back; a higher, 200,000 years; a lower, 300,000; a rather low minimum 410,000; a low maximum, 475,000; a very low minimum, 520,000; a maximum, 570,000; two maxima, the second 750,000; a very low minimum 800,000; an extreme maximum, 850,000; another very low minimum, 900,000; a high maximum, 950,000; etc.—In future time, there will be a very low minimum, 24,000 years on; a low maximum, 150,000 years; another low maximum, 250,000; a very low minimum, 300,000; a low maximum, 400,000; a very high maximum, 515,000; a minimum, 560,000; an extreme maximum, 610,000 years; and so on. Some of the minor undulations, and most of the minima, are not here noted.

2. *Note on an Electrification of an Island;* by F. JENKIN.—A curious discovery has been made by Mr. Gott, the superintendent of the French company's telegraph station at the little island of St. Pierre Miquelon. There are two telegraph stations on the island. One, worked in connection with the Anglo-American company's lines by an American company, receives messages from Newfoundland and sends them on to Sydney, using for the latter purpose a powerful battery and the ordinary Morse signals.

The second station is worked by the French Trans-atlantic Company, and is furnished with exceedingly delicate receiving instruments, the invention of Sir William Thomson, and used to receive messages from Brest and Duxbury. These very sensitive instruments were found to be seriously affected by earth-currents; i.e., currents depending on some rapid changes in the electrical condition of the island; these numerous changes caused currents to flow in and out of the French company's cables, interfering very much with the currents indicating true signals. This phenomenon is not an uncommon one, and the inconvenience was removed by laying an insulated wire about three miles long back from the station to the sea, in which a large metal plate was immersed; this plate is used in practice as the earth of the St. Pierre station, the changes in the electrical condition or potential of the sea being small and slow, in comparison with those of the dry rocky soil of St. Pierre. After this had been done, it was found that part of the so-called earth-currents had been due to the signals sent by the American company into their own lines, for when the delicate receiving instrument was placed between the earth at the French station and the earth at the sea, so as to be in circuit with the three miles of insulated wire, the messages sent by the rival company were clearly indicated, so clearly indeed, that they have been automatically recorded by Sir William Thomson's syphon recorder. Annexed is a facsimile of a small part of the message concerning the loss of the steamship Oneida, stolen in this manner [here omitted.]

It must be clearly understood that the American lines come nowhere into contact, or even into the neighborhood of the French

line. The two stations are several hundred yards apart, and yet messages sent at one station are distinctly read at the other station; the only connection between the two being through the earth; and it is quite clear that they would be so received and read at fifty stations in the neighborhood all at once. The explanation is obvious enough: the potential of the ground in the neighborhood of the stations is alternately raised and lowered by the powerful battery used to send the American signals. The potential of the sea at the other end of the short insulated line remains almost if not wholly unaffected by these, and thus the island acts like a sort of great Leyden jar, continually charged by the American battery, and discharged in part through the short insulated French line. Each time the American operator depresses his sending key, he not only sends a current through his lines, but electrifies the whole island, and this electrification is detected and recorded by the rival company's instruments. * * *

All owners of important isolated stations should use earth-plates at sea, and at sea only. This plan was devised by Mr. C. Varley many years ago to eliminate what we may term natural earth-currents, and now it should be used to avoid the production of artificial earth-currents which may be improperly made use of.—*Nature*, May 5th, 1870.

3. *Baron von Richthofen's Explorations in China.*—The Baron v. Richthofen, formerly of the Geological Survey of Austria, accompanied Mr. J. Ross Brown to China two years ago, and since then has occupied his time in making geological explorations in China. His investigations have been encouraged by the generous subscription of 16,000 dollars, made by the American merchants of China to aid him in prosecuting such exploration. An interesting feature of this subcription is that he is at liberty to use it as he deems best for the interests of science, without regard to immediate commercial or economic results, and in addition to his geological work, he purposes to institute meteorological observations at several points.

Already many important additions have been made to the knowledge of the geology of Northern China and Manchuria. Some of his observations made between Shanghai, and Han-kau were published in the Proceedings of the American Academy of Boston, vol. viii. Since these were written other letters have been received by Prof. J. D. Whitney, from which we hope to make extracts for a future number of this Journal.

4. *Thermal Units.*—Prof. T. Muir proposes in "Nature" of April 14th, the introduction of the word *therm* for a thermal unit, making that unit *the quantity of heat necessary to raise the temperature of* 1 *gram of water from* 0° C. *to* 1° C. *Therm*, *hectotherm*, *kilotherm* would be consequently the expressions for respectively 1,100, 1000 therms, suggestively corresponding to gram, hectogram, kilogram in name as well as in nature."

5. *Astronomer at the Cape of Good Hope.*—Mr. E. J. Stone, F. R.S., of the Greenwich Observatory, has received an appointment to this position.—*Nature*, June 23.

6. *American Association.*—The American Association for the Advancement of Science will hold its next meeting in Troy, commencing on the first Wednesday in August. Prof. William Chauvenet is the President for the year.

7. *British Association.*—The next meeting of the British Association will be held at Liverpool on Wednesday, the 14th of September, under the Presidency of Prof. Huxley.

8. Admiral Russell Henry Manners, President of the Astronomical Society of London, died recently, aged 70 years.

IV. MISCELLANEOUS BIBLIOGRAPHY.

1. *A concise analytical and logical Development of the Atmospheric System as God made it; and the Elements of prognostication by which the weather may be forecasted; designed as a Weather-book for the practical mind of the Country;* by Thomas B. Butler. Hartford, 1870. 404 pp. 8vo.—This work on Meteorology will be read with profit by all interested in the subject. It presents a large number of facts of the Author's own observations, together with others from various sources, and is illustrated with many woodcuts. The title is calculated to excite a prejudice in scientific minds against the volume, and many would at once substitute for "The Atmospheric System as God made it," "The Atmospheric System as Thomas B. Butler makes it." The subject is too large a one to be discussed in a book notice; and we refer, therefore, to the volume for the views, which, according to the motto on the title page, is, in the author's opinion, "the truth [that] is mighty and will prevail."

2. *Synopsis of the Extinct Batrachia and Reptilia of North America*: Part 2d. By Edward D. Cope.—This second part of Prof. Cope's important work on the Extinct Batrachia and Reptilia of North America includes the synopsis of the remainder of the Dinosauria, the Testudinata, the Pterosauria, the Pythonomorpha, and the Ophidia. The Testudinata are divided into two groups: the Cryptodira, represented by thirty-four species which are referred to fourteen genera, and the Pleurodira, represented by ten species of four genera. *Rhabdofelix longispinis* Cope is the only known North American representative of the Pterosauria. Of the Pythonomorpha, the author mentions twenty-seven species. Three species of the Ophidia are given. Several additional Reptilian species are remarked upon in the Appendix, and one new genus of the Cheloniidæ is defined. Prof. Cope, through his labors, is throwing great light on the ancient vertebrate life of this continent.

3. *First Principles of Chemical Philosophy;* by Josiah P. Cooke, Jr., Erving Professor of Chemistry and Mineralogy in Harvard College. 534 pp. text and 22 tables and index, 12mo.—The first part of Prof. Cooke's Chemical Philosophy was noticed in a former volume of this Journal. That portion of the work, it will be remembered, was devoted to the development of the fundamental principles of chemical science. In the second part of his treatise, the author gives a succinct, but very comprehensive, summary of the more important elements and compounds, exhibiting

their constitution and relation by means of formulæ and reactions, illustrated by problems calculated to direct the attention of the student to the more important facts and principles of the science.

By his method of developing the subject, Prof. Cooke succeeds in giving to chemistry a high degree of logical sequence and mathematical exactness, and his work, taken in connection with a good book of laboratory practice, will be found a most efficient aid to both teacher and pupil in the study of chemistry.

4. *Neue Untersuchungen über den elektrisirten Sauerstoff;* von Dr. G. MEISSNER. Mit zwei lithographirten tafeln. Göttingen, 1869. 4to, pp. 110.—This brochure, re-published from the Transactions of the Royal Academy of Sciences in Göttingen, contains the experiments made by the author, with the aid of his previous experience, in support of his opinion that the so-called atmizone or antozone fog consists only of electrified oxygen and water; an opinion stated in his previous research published in 1863. An abstract of this supplementary memoir of Meissner will appear in our next number.

5. *Great Outline of Geography: a Text-Book to accompany the Universal Atlas;* by THEODORE S. FAY. 238 pp. 12mo. New York, 1869. (G. P. Putnam & Son).—Mr. Fay has made an important contribution to our means of instruction in geography in the Atlas and its accompanying text, lately published by Putnam & Son. The work has been long in preparation, the map having been prepared by Hassenstein of Berlin, during the author's long residence as American Minister at European courts. A fac-simile of a letter received by the author from Humboldt expressing his approval of it, is given in the volume. The number of plates constituting the atlas is eight, the most of them double and all well executed. They give, as far as the limited space allows, the chief astronomical, political and historical features of geography, together with some of the physical, and are presented in a clear and striking manner. The text consists partly of brief explanations of terms and principles, and of historical, geographical and other observations, but mainly of lists of places to be looked out on the maps. The plan of instruction proposed in the work is for the teacher to read the lesson from the book while the pupil before him "follows every word upon the plates," looking for the cities, rivers, &c., as the names are mentioned; and this exercise, which is not to exceed, at any one time, an hour, is study enough. The pupil who goes through with the work under the guidance of a patient teacher cannot fail to become well acquainted with the subject.

6. *Geology and Revelation; or the ancient history of the earth considered in the light of Geological facts and revealed religion, with Illustrations;* by the Rev. GERALD MOLLOY, D.D., Professor of Theology in the Royal College of St. Patrick Meynooth. 418 pp. 12mo. London, 1870. (Longmans, Green, Reader and Dyer).—No book on this subject has appeared from the theological side that is more worthy of respect than this by Dr. Molloy. The author's discussion of the principles of geology, with reference to

their bearing on the Mosaic history of Creation evinces great familiarity with the science and the necessities of the unscientific reader, and thorough knowledge of the sacred record. In treating of the antiquity of the earth he enforces his argument by copious quotations from the Christian Fathers, showing that long before geology had any existence as a science, and of course, when the discussions and doubts it has excited were unknown, the essential points respecting *time* and the order of Creation had received careful attention from devout thinkers, and that the conclusions, at which they arrived on purely theological grounds, were in most cases much the same as those which the best writers of our time deduce from geological evidence. Dr. Molloy closes his arguments as follows:—"We have then two distinct systems of interpretation according to which the vast antiquity of the earth asserted by geology may be fairly brought into harmony with the history of creation recorded in Scripture. One allows an interval of incalculable duration between the Creation of the Heavens and the Earth, and the work of the six days; the other supposes each one of the six days to have been itself an indefinite period of time." The questions touching the antiquity of the human race the author proposes to discuss separately at a future time.

We understand that this work will soon be issued from the publishing house of G. P. Putnam & Son of New York.

7. *Reliquiæ Aquitanicæ, being contributions to the Archæology and Palæontology of Périgord and the adjoining Provinces of Southern France;* by E LARTET and HENRY CHRISTY, edited by THOMAS RUPERT JONES, Prof. Geol., etc., Roy. Mil. College, Sandhurst. Part X, for February, 1870, carries the work to pages 140 and 132 in its two parts, the plates to A XXXII, and B XVIII.

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